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PROGRESS IN SCIENCE.

Including Proceedings of Learned Societies at Home and Abroad, and Notices of Recent Scientific Literature.

THE QUARTERLY
JOURNAL OF SCIENCE.
JANUARY, 1874.

I. THE SATURNIAN SYSTEM.

By RICHARD A. PROCTOR, B.A. (Cambridge).
Author of "Saturn," "The Sun," "The Moon," &c.

IT has always appeared to me, since first I studied the subject of Saturn and his system, that our books on astronomy fail to indicate effectively the position which this noble planet, and the scheme over which he holds sway, bear in the economy of the solar system. The remark extends to Jupiter, and in a less degree to Uranus and Neptune. There is to my mind something most incongruous between the true teachings of astronomy respecting the giant planets, and the notions complacently presented in book after book on elementary astronomy, and even in treatises by masters of the subject. We have the astronomy of the ancients and the modern astronomy intermixed. We see rightly taught the dimensions and general aspect of the different planets, but we find these bodies classed together precisely as they very reasonably were when astronomers knew little more than that there are, besides the sun and moon, the planets Mercury, Venus, Mars, Saturn, and Jupiter. The orbits of these bodies are plotted down in a series of concentric and equidistant circles, on which very commonly are shown pictures (save the mark) of the several planets, and the reader is left to combine the utterly erroneous notions thus indicated with such ideas as he may derive from the array of numbers contained in the tables of planetary elements. Nor in the verbal description of the planets is any stress laid upon the characteristic peculiarities which distinguish the outer family of planets from the inner. Differences are stated, but mere statement in such cases counts for very little; the impression really conveyed is, that whereas the earth, and Mars, and Venus, and Mercury are so many smaller worlds, Jupiter, and Saturn, and Uranus, and Neptune are as many larger worlds; and whatever peculiarities distinguish these outer and larger planets from the rest are discussed with direct

reference to familiar terrestrial relations. For instance, if the satellites of Saturn are referred to, the remark is made that the external skies of Saturn must be well illuminated, and very beautiful with all these moons, when our own single moon forms so beautiful a feature of our nights. When the rapid rotation of Saturn is mentioned, we are told immediately that therefore the day in Saturn lasts only so many of our hours. So with the Saturnian year, because the mean density of Saturn is but about 13-100ths of the earth's; we are told that, therefore, his globe is constructed of material as light as mahogany; and so on, through a variety of such comparisons.

It appears to me that the history of investigations into the physical condition of the planets is characterised by a very singular unreadiness to view the whole subject from the new standpoint, available when telescopic observations began to be made. The new knowledge gained by means of the telescope was welded to the old ideas respecting the planets. New cloth was added to old garments. The natural course, one would have supposed, would have been to consider the planets altogether in the light of information obtained by the telescope, simply because that was the first really reliable information obtained by astronomers. Everything until then had been guess work; yet the results of such guess work still appears in our books on astronomy. It is not Saturn or Jupiter, as revealed by the telescope, with which our writers on astronomy deal; but they tell us, in effect, that the Saturn and Jupiter of the old astronomers have been found to have such and such dimensions, rotation-rates, aspect, and so on.

Accordingly, the attempt to reason respecting these planets in perfect independence of the ideas entertained ages ago by astronomers, is regarded as a species of innovation. That is held to be rash and fanciful speculation which in point of fact is the only scientific way of treating the subject. It is held to be a sort of heresy to speak of Saturn or Jupiter in terms not strictly compatible with the words of Sir W. Herschel, for example, although his ideas respecting these planets were based entirely on the ideas formerly entertained about them, and conveyed to Sir W. Herschel through the instructive but not very suggestive teachings of Ferguson. I have not, indeed, myself had to complain on this point. I have, in fact, been surprised at the exceedingly liberal manner in which my own theoretical opinions have been received—I may even say welcomed—by many who, nevertheless, still retain a prejudice for the older way of

treating the subject. But I observe that other writers have been less fortunate; and when they have indicated the significance of the features which distinguish giant planets from the terrestrial planets, and touch on some of the conclusions flowing from such considerations, their views are scouted as wild and fanciful—too startling, in fact, to obtain acceptance among men of science.

In passing, I would touch on the objection to startling views, merely because they are startling, as utterly unreasonable in the presence of all that has been discovered, not merely in astronomy, but in science generally. The accepted truths of science are nearly always startlingly unlike what has been imagined before the evidence became strong enough, or was well studied enough, to indicate the real facts. Take, for instance, what has been learned recently about the sun. Suppose that twenty years ago the now known truths about the constitution of the sun, about coloured prominences, and about the corona had been simply enumerated, either as the result of theoretical considerations or of observations the nature of which was not made known: then it is certain that the surprising character of these new views would have rendered them unwelcome to astronomers. Less startling theories would have been received with favour, and quoted as altogether preferable to notions so bizarre and fanciful; yet the sober theories of twenty years ago were wide of the truth. They were in reality fanciful, and though not far fetched, yet ill fetched; drawn, in fact, from utterly incorrect analogies.

It may be assumed, indeed, ordinarily, that the relations as yet unknown are such as we should regard as surprising. It is not merely an unsafe, but an almost certainly misleading assumption that the best explanation of facts is the one which is most obvious and natural. What can be more natural, for instance, than the theory that the solar corona is due to the passage of the sun's rays through our own atmosphere?—what more utterly misleading? How natural was Faye's theory that the solar sierra is a phenomenon produced by the lunar atmosphere; and yet the theory was altogether and demonstrably unsound. To go farther back in the history of astronomy, the Ptolemaic theory was unquestionably intended to explain in the most natural way the celestial motions observed from our earth, obviously fixed, obviously far larger than any of the heavenly orbs. Even more natural and seemingly obvious is the theory that the earth is a great plain. The true theory in

these and hundreds of other instances has not been the theory which commended itself by its obviousness and by its close accordance with what was mistakenly regarded as the *natural* order of things.

I propose in the present essay on the Saturnian system to begin with the consideration of known facts about Saturn and his system, and from them to endeavour to educe just ideas respecting the constitution of the Saturnian system, proceeding in perfect independence of all preconceived opinions. I shall neither endeavour to support nor to overthrow the common notion that Saturn is a globe resembling our earth in nature, though larger and in certain details unlike the earth. I shall endeavour as far as possible to treat my subject as though the earth were not the sole orb in the universe with which we have a close acquaintance.

In the first place, then, let us consider Saturn's position in the solar system, as respects mass, the most important element of a planet's condition, simply because the mass of a planet measures the planet's power.

Regarding the solar system as a whole, we see the sun so largely surpassing all the planets together in mass and volume, that if we knew nothing else respecting him, we should recognise the fact that he belongs to another order of created things. He does not surpass the several planets only, but all the planets taken together.

But we are apt to overlook the fact that Jupiter and Saturn are as markedly distinguished from the earth and Venus as the sun is from the giant planets. Jupiter alone surpasses all the members of the inner or terrestrial family of planets, taken together, about a hundred and forty times. Saturn surpasses them all, taken together, about forty times. A difference such as this can hardly be described as one of degree merely. It is a difference of kind, so far as mass *can* indicate such a difference. It seems a sufficient proof of this to note that if the sun were destroyed as well as Saturn, Uranus, and Neptune, then Jupiter could effectively replace the sun as a ruling orb, round which the terrestrial families could travel, not indeed on such wide orbits as at present, but on paths of great extent, Jupiter remaining as appreciably stable at the centre of the scheme as the sun now is. The like can be said about Saturn, since his mass exceeds that of the earth—the largest member of the family of minor planets—about ninety times.

Even taking into account Uranus and Neptune, Jupiter and Saturn are supreme among the members of the solar

system. Jupiter is indeed easily first, seeing that he surpasses about $2\frac{1}{2}$ times (in Mars) all the other members of the family, including Saturn, taken together. But Saturn is as easily second, seeing that he in turn surpasses all the other members of the planetary scheme, taken together, nearly three times.

Still it is, when we compare either Jupiter or Saturn with the members of the terrestrial family of planets, or rather with that family as a whole, that we perceive most clearly that those giant planets belong to another order of bodies. Our earth does not to the same degree surpass the family of asteroids regarded as a whole. It is indeed rather surprising that the asteroids should so readily have been recognised as belonging to a distinct order, while the giant planets, which differ fully as much from the terrestrial family of planets as these planets differ from the asteroids, should be regarded as though they were members of one and the same family.

It may be urged, perhaps, that the asteroids by travelling altogether within a comparatively small region of the sun's domain seem marked out as forming a distinct family. But in truth, the region within which the asteroids pursue their path is very much larger than that occupied by the members of the terrestrial family of planets—certainly twice as large, if we leave altogether out of account the great range of the asteroids in distance from the mean plane of the solar system. The orbital range of the terrestrial family of planets is indeed so small, compared with that of the outer family, that the two sets of orbits cannot be properly represented in the same diagram. If a diagram includes the orbits of the giant planets properly drawn to scale, then the path of Mars, the outermost of the terrestrial planets, is represented by a circle considerably smaller than that commonly used to represent the sun in pictures of the solar system.

Let us endeavour, then, to picture to ourselves the giant globe of Saturn pursuing its career so far from the central orb that the region girt round by its orbit exceeds more than ninety-fold the region which the earth circuits in her annual revolution. Let us remember that in precisely the same degree the solar light and heat are reduced at Saturn's distance, and (also in the same degree) the attractive influence of the sun, and singularly enough, in the same degree, Saturn's own attractive influence exceeds the earth's. For the former relation was a necessary consequence of the law according to which light, heat, and gravity diminish

with distance;* but the latter is, as it were, accidental, since there is no necessary connection between the mass of any planet and the distance at which it travels. It is worthy of notice, as a convenient aid to the memory, that while the area swept out by Saturn is about ninety times that swept out by the earth, and solar heat, light, and gravity at Saturn about one-ninetieth less than at the earth, the mass of Saturn exceeds that of the earth about ninety-fold. It is manifest, then, that Saturn must be regarded as a much more efficient ruler over the region of space along which he circles than the earth can possibly be over the region of space through which she travels. Or rather, it is manifest that the range of Saturn's influence is much wider. So feeble is the earth, so narrow is the sphere of her special influence, that even her own moon is far more fully under solar influence than under terrestrial rule. In fact, if we remember that the sun's mass is about 315,000 times that of the earth, we see that his influence equals hers at any point whose distance from the sun is to that from the earth as the square root of 315,000 to unity, or about as 561 to 1. Now since the distance of the earth from the sun is about 91,500,000 miles, a point on the line joining the earth and sun (and between these bodies) must be distant from the earth 1-562nd part of this distance to be equally influenced by the sun and earth. This distance is rather less than 163,000 miles, whereas the moon's distance from the earth amounts to 238,800 miles. A point beyond the earth on the line joining the sun and earth produced should be at 1-560th part of 91,500,000 miles from the earth, to be equally influenced by the earth and sun, that is about 164,000 miles. This is the farthest point from the earth at which her influence equals that of the sun; and if a sphere be described in space at any instant, having the line joining this last-named point and the one before determined (163,000 miles from the earth's centre *towards* the sun) as diameter, then at all points within this sphere with its diameter of 327,000 miles or thereabouts, the earth acts more potently than the sun; but everywhere else she acts less potently. Now let us apply a similar process to Saturn, in order to ascertain the dimensions of the sphere over which his power is, as it were, supreme, and

* Since the area swept out by a planet in completing its orbit varies directly as the square of the mean distance, while light, heat, and gravity vary inversely as the square of the mean distance, it follows that the light, heat, and attraction influencing bodies revolving around the same central sun vary inversely as the area of their orbits.

we shall at once perceive how different is his position as a ruler of matter compared with that of the earth. The mean distance of Saturn from the sun amounts to 872,137,000 miles, and his mass is about 90 times the earth's, or about 1-3500th of the sun's mass. Hence the sun's influence equals Saturn's at any point whose distance from the sun is to that from Saturn as the square root of 3500 to unity, or about as 59 to 1. Hence a point on the line joining Saturn and the sun, and between these bodies, must be distant from Saturn 1-60th part of the distance of Saturn from the sun to be equally influenced by the sun and Saturn. This distance is about 14,500,000 miles from Saturn on the side towards the sun. On the farther side, a point equally influenced by Saturn and the sun would lie at a distance from Saturn equal to 1-58th part of Saturn's distance from the sun, or at a distance of about 15,000,000 miles. Hence the sphere over which Saturn bears supreme sway has a diameter of 29,500,000 miles. We have seen that the sphere over which the earth bears supreme sway has a diameter of only 327,000 miles. Hence Saturn's sphere of rule is to the earth's, in diameter, as 29,500 to 327, or is about 90 times greater.* Here strangely enough the proportion 90 to 1 comes in yet again, not as the reader might imagine at a first view, as a necessary consequence of the same proportion 90 to 1 in the mass of Saturn compared to the earth's, and in the square of his mean distance to the square of the earth's, but independently—since it would not have appeared as the result of our calculations did not the sun's mass bear to Saturn's the proportion 3500 to 1. The *volume* of the sphere ruled over by Saturn bears to the volume of the sphere ruled over by the earth a proportion of about 730,000 to unity; and it

* It may be interesting to determine in the same way the extent of the sphere over which Jupiter bears sway. His mean distance from the sun amounts to 475,692,000 miles, and his mass is equal to about one $\frac{1}{1048}$th part of the sun's. Hence the influence of Jupiter and the sun are equal at any point, whose distance from Jupiter is to its distance from the sun as 1 to the square root of 1048, or as 1 to about 32. Hence we must take from Jupiter on the side *towards* the sun a distance equal to $\frac{1}{33}$rd part of Jupiter's distance, and on the side away from the sun a distance equal to $\frac{1}{31}$st part of Jupiter's distance. These distances are respectively about 14,400,000 miles, and about 15,300,000 miles; so that the sphere over which Jupiter bears superior sway has a diameter of about 29,700,000 miles, which may be regarded as about equal to the diameter of the sphere over which Saturn bears superior sway, I have spoken of the region over which a planet bears special sway as a sphere, and this is actually the case. It is manifest from the reasoning that the planet is not centrally placed within the sphere, but is nearer the side towards the sun. The sphere is manifestly not constant in dimensions, being larger or smaller, according as the planet is farther from or nearer to the sun.

may not unfairly be said that this proportion indicates the relative position of the two orbs—Saturn and the earth—as respects power in the scheme of the planets. This proportion seems certainly more truly to indicate their relative power than the direct proportion between their masses—since we must recognise in the earth's relative proximity to the sun a source of comparative inferiority as a ruler over matter. It is noteworthy, moreover, that if we adopt this criterion, Saturn and Jupiter are brought almost to equality, notwithstanding the greatly superior mass of the last named planet.

It is to be noticed that the relation here considered bears very importantly on the question of the original formation of the planetary system. I must admit that, for my own part, I find much in Laplace's conception of the genesis of the solar system, which is very far from satisfactory. A gradually contracting nebulous mass, such as he pictures, could scarcely in my opinion have produced a system in which the masses are at a first view so irregularly scattered as in the solar system. But if we adopt such a view as I have endeavoured to maintain in my "Other Worlds," we find in the position of the various members of the solar system a satisfactory reason for their various dimensions. According to that view the solar system had its origin in the gathering together of matter towards a great centre of aggregation. The subsidiary centres of aggregation which would as naturally arise during such a process as subsidiary whorls in a gigantic whirlpool, might be expected to have such dimensions as we actually observe. Close to the great centre, such centres would be relatively small, because the ruling centre would be supreme everywhere within the sphere close around him, except quite close to subordinate aggregations. No matter except such as passed very close to such aggregations would be gathered in. We may suppose that such aggregations would indeed only form on account of the enormous wealth of matter within that sphere, and the consequent certainty of collisions, or very close approaches resulting in agglomeration; and towards the outskirts of this part of the sphere of the sun's influence there would not even be any definite agglomeration, but a number of very small—and, as it were, accidental—aggregations resulting in the ring of asteroids. Outside the sphere of the sun's overmastering influence, there would still be a great wealth of matter, and gathering aggregations would exert their attractive energies far more widely. Nearest of all to the central region would come

the mightiest aggregation of all, on account of the greater quantity of matter. Here, then, Jupiter had his birth, chief giant of the solar system, and prince of all the planets. But the range of sway would widen with increasing distance, and at first such widening would go far to counterbalance the gradual falling off in the gravity of matter. Moreover, with increase of distance from the sun would come a greater freedom to form out of the aggregating matter subordinate schemes or systems. Next, then, beyond Jupiter, with his giant bulk and uniform system of secondary bodies, came Saturn into being, inferior in mass to his giant brother, but ruling over a wider space, separated from Jupiter by a far greater distance than separates Jupiter from the asteroids, and in fact, according to the criterion I have laid down above, nearly the equal of Jupiter in the range of his independent power. Here we see the origin of the most remarkable system within the solar domain—a scheme of rings whose complicated structure is as yet not half understood, and a family of eight satellites as diverse in size and arrangement as the eight primary members of the solar family itself; and we can readily understand how, with yet increasing distances and continually decreasing quantity of matter, the minor but still gigantic masses of Uranus and Neptune should appear on the outskirts of the solar system. I venture to predict, with some degree of confidence, that if any trans-Neptunian planet be ever discovered, it will be inferior, but not greatly inferior, to both Uranus and Neptune in mass.*

Let us now, however, turn from the consideration of the mass and weight of Saturn, and of the processes by which he may be thought to have reached his present condition, to the inquiry what that condition probably is.

We see Saturn, then, the centre of a scheme of wonderful extent and importance. The outermost satellite circuits in an orbit having a diameter of more than 4,600,000 miles. This is the widest span of any satellite orbit, and not far short of ten times the span of our moon's orbit. The widest span of the Jovian satellite system amounts to but 2,380,000 miles, or almost exactly five times the span of the moon's orbit. It will be observed that both the Saturnian and

* In old times this would have been a tolerably safe prediction, since any such planet would have been very unlikely indeed to be discovered. But in these days, when the zodiac is continually being swept for the discovery of new planets, and when new planets are being detected at an average rate of about five per annum, it is exceedingly unlikely that any trans-Neptunian planet will long escape detection, supposing any such planet exist to be detected.

Jovian satellite systems lie far within the limits of the spheres ruled over with superior sway by these planets respectively. For it will be remembered that Saturn's sphere of sway has a diameter of about 29,500,000 miles, while Jupiter's (see note at p. 7) is about 200,000 miles wider. The effect of this is seen in the motions of the Jovian and Saturnian satellite systems. For whereas the orbit of our moon around the sun is everywhere concave towards the sun, notwithstanding the motion of the moon round the earth, the paths even of the outermost of the Jovian and Saturnian satellites are markedly convex towards the sun when these bodies are in inferior conjunction. The inner satellites, in fact, not only move at those times on courses convex towards the sun, but have an excess of motion in their orbit round their primary over the onward motion which they share with him around the sun, and consequently travel backwards in this part of their motion. It follows that in successive lunations the inner satellites trace out looped paths. This happens with the four satellites Mimas, Euceladus, Tethys, and Dione. The fifth, Rhea, shows the singular peculiarity of coming almost exactly to rest when in inferior conjunction—that is, to rest relatively to the solar system. The path of this satellite with reference to the solar system is nearly an epicycloid, any portion of which, from cusp to cusp, is appreciably a cycloid. The satellites Titan, Hyperion, and Japetus follow waved courses convex to the sun through a considerable portion of each lunation. It will be seen that Saturn has his system in complete control.* Even the outermost is very much less perturbed by the sun (relatively as well as absolutely) than our moon.

The second satellite inwards has an orbit diameter of rather more than two millions of miles. It is a somewhat remarkable circumstance that this satellite, lying between Titan and Japetus, the two largest satellites, should be the smallest or at least the faintest of all Saturn's satellite

* The distinction between our moon and the satellites of the major planets is a marked one, and significant. Our moon is, in point of fact, much more to be regarded as a fifth planet of the inner family of terrestrial planets than as an orb dependent on our earth. If she could be watched from a distant standpoint, her motions would scarcely indicate the fact that she circles around the earth; for, in fact, the result of this so-called circling motion corresponds simply to large perturbational effects. On the contray, the motions of the satellites of Jupiter, Saturn, Uranus, and Neptune, are chiefly influenced by their respective primaries, and this could not but be recognised if the motions of these moons were watched from a distant point. Even if their primaries were concealed during the observation, the motions of the satellites would reveal the fact that these bodies are ruled by a central orb.

family. One would be led to suspect that Hyperion is but one member of a *zone* of small satellites travelling between the paths of Titan and Japetus. It appears to me to be a confirmation of this view that the path of Hyperion does not correspond with the general arrangement of the scheme, but bears somewhat the same sort of relation to it that we should recognise in the orbit of one of the innermost of the asteroids if taken, instead of the *zone* of asteroids, to represent the orbit intermediate to the paths of Mars and Jupiter.

As we proceed onwards towards Saturn we are struck with the comparatively close order of the orbits of the inner satellites. The distance separating the orbit of the innermost from that of the fifth Rhea is less than half the distance separating the orbit of Rhea from that of Titan the sixth. The following table of distances has been calculated on the assumption that the sun's equatorial horizontal parallax is 8·916″ :—

	Name.	Distance in mean radii of Saturn.	Distance in miles.	Difference. in miles.
I.	Mimas	3·3607	115,335	
II.	Euceladus	4·3125	148,000	32,665
III.	Tethys	5·3396	183,250	35,250
IV.	Dione	6·8395	224,740	41,490
V.	Rhea	9·5528	327,840	103,100
VI.	Titan	22·1450	759,990	432,150
VII.	Hyperion	26·7834	919,170	159,180
VIII.	Japetus	64·3590	2,208,720	1,289,550

Passing yet farther inwards, after crossing the orbit of Mimas, we come upon the ring-system. The outer edge of the outer ring lies at a distance of 83,460 miles from the centre of Saturn, or 31,875 miles from the orbit of the innermost satellite. It is noteworthy how uniformly the distances from this ring outwards to orbit after orbit of the four inner satellites proceed. This uniformity somewhat resembles what we notice in the case of the four terrestrial planets, since we see that the distance from the sun to Mercury, thence to Venus, and thence to the earth, are very nearly equal (being roughly 35, 32, and 35 millions of miles), while the distance to Mars, though greater, belongs to the same order of distances. The remaining elements, which are convenient for reference in treating of the ring-system, are the following :—

Exterior diameter of outer ring in miles . .	166,920
Interior ,, ,, ,, . .	147,670
Exterior ,, inner ring ,, . .	144,310
Interior ,, ,, ,, . .	109,100
Interior ,, dark ring ,, . .	91,780
Breadth of outer bright ring	9,625
Breadth of division between rings	1,680
Breadth of inner bright ring	17,605
Breadth of dark ring	8,660
Breadth of system of bright rings	28,910
Breadth of entire system of rings	37,570
Space between planet and dark ring	9,760

We are thus brought to the planet's globe. Its mass we have already indicated. Its dimensions are as follows:—the equatorial diameter is about 70,150 miles, the polar diameter about $\frac{2}{19}$ths less, or about 63,500 miles. The volume exceeds the earth's 697 times, so that since in mass Saturn only exceeds the earth about 90 times, his mean density is but about $\frac{10}{77}$ths of the earth's. His globe rotates in about 9h. $55\frac{1}{2}$m. on an axis inclined about $26\frac{3}{4}°$ to the perpendicular to Saturn's orbit-plane. The rings and all the satellites, except the outermost, travel nearly in the plane of Saturn's equator, but the outermost satellite travels on a path inclined about 15° to that plane.

Even on a general survey, only, of this wonderful system, the impression conveyed to the mind would not be that we have in Saturn an orb belonging to the same order as our earth, were it not for the influence of the preconceptions to which I have already adverted. It seems to me that if we could imagine a visitant from outer space viewing our solar system, he would at once recognise in Jupiter and Saturn the members of an order of orbs probably intermediate in character between the sun and the minor planets. He would reason that while, on the one hand, these planets being very much less than the sun in volume and mass, are obviously of an inferior order, and in this sense resemble the earth and Venus; they are, on the other hand, so far comparable with the sun, even in these respects, that they largely exceed the earth and her fellow planets—that is, in those circumstances in which alone the major planets are comparable with the minor planets, they are as far removed from them on the one hand as from the sun on the other. But when we turn to features in which the major planets resemble the sun, we find that they differ absolutely from the minor planets. Thus, in having the complete control of systems

of bodies, these planets resemble the sun, and are utterly unlike the earth. In mean density Jupiter is almost exactly like the sun, and Saturn has even a less mean density than the earth. This last is the most important distinction of all, as we shall presently see; in fact, I think I shall be able to show that of itself it demonstrates the fact that the major planets are utterly unlike the earth and her fellow minor planets. Again, in the condition and phenomena of their atmospheric envelopes, Jupiter and Saturn to a certain degree resemble the sun, as will presently appear; and though the resemblance is not altogether complete, yet this is counterbalanced by the circumstance that the want of resemblance between the major and minor planets in this respect is very marked indeed.

It will be understood that I rely in the main for evidence as to the condition of the atmosphere of Jupiter and Saturn on the results of telescopic researches. Other evidence there is, and in particular the spectroscope has afforded information of an interesting nature. But as yet this information is not definite enough to be reliable as a basis of reasoning; whereas the evidence given by the telescope is sufficient, rightly used, to convey very important information. I would note that we may properly combine the information given by both Jupiter and Saturn, since these planets manifestly resemble each other in those leading features which we can alone deal with in our present enquiries. It is altogether likely that in minor respects Jupiter and Saturn are as unlike as the earth and Mars. But precisely as we can trace a general resemblance between all the members of the minor family of planets, so manifestly Saturn and Jupiter (as also probably Uranus and Neptune) resemble each other in the broader features of their condition. It is very important to recognise this, because, in point of fact, the information conveyed by one planet supplements in an interesting way that given by the other. Jupiter being very much larger and far nearer to the earth can be more satisfactorily studied; and we can only recognise, in the case of Jupiter, the details of those atmospheric belts which girdle both planets. But on the other hand Saturn affords a test as to the nature of these belts, which is wanting in the case of the larger planet. For Jupiter's equator plane is very little inclined to the level in which the planet travels, whereas, as has been mentioned in the elements given above, the equator plane of Saturn is inclined at a very considerable angle, so that what we may for convenience term the seasonal changes of Saturn

(noting always, however, that they must be quite unlike the seasons of our earth) are of a marked character, whereas Jupiter has scarcely any seasonal changes at all. Here, then, is a criterion to show what part, if any, the sun plays in producing changes in the condition of the atmospheric envelopes of these planets, since we should expect, if his action is the leading cause of changes, that the changes in the Jovian belts would differ markedly in character from those in the Saturnian belts.

I take for granted the ordinarily described telescopic features of the belts, as presumably known to all the readers of this Journal.

The first feature to be noticed as bearing on the condition of the atmospheres of Jupiter and Saturn, is the remarkable parallelism of the belts. It has been commonly stated that this feature is comparable with the existence of trade-wind zones on our earth. I apprehend that if our earth were viewed from Venus or Mercury, even with high telescopic power, nothing like zones would be recognised. It is, indeed, only over the oceans that the equatorial cloud band exists; and even this is but a mid-day phenomenon. The sun rises in a clear sky in equatorial regions at sea; and it is only towards noon that heavy clouds cover the whole sky. In the afternoon these are dissipated by heavy rain-falls and electric storms, and towards sunset the sky is clear.

There is, indeed, a difficulty in accounting for the zones in the atmospheres of Jupiter and Saturn. Their ordinary regularity implies the existence of a much more effective cause than that which produces our trade-winds. The cause *must* be, it should seem, a difference of rotational velocity, causing clouds to lag as clouds on our trade zones do, only much more markedly, or else causing clouds to be carried in advance of the prevailing rotation-rate. Either cause would serve equally well; the difficulty lies in understanding how either can operate with sufficient activity. The rapid rotation-rates of Jupiter and Saturn would of course make the differences of rate correspondingly great. But so far as the cause which produces our own trade-winds is concerned, this circumstance is much more than counterbalanced by the greatly reduced effects of solar action in Jupiter and Saturn. That, notwithstanding this reduction of effect, sun-raised clouds should be so much more energetically swayed into zones than on our earth must be regarded as altogether improbable. We must find some other interpretation of these zones,—a result which would indeed have been forced upon us, as I think, by the mere

circumstance that there are sometimes so many of them. Polar and equatorial air-currents such as exist in our own air would naturally explain, perhaps, an equatorial cloud-zone and sub-tropical trade zones on either side of it; but they can afford no explanation of the existence, even though occasional only, of three or four well-marked zones on either side of the equatorial one.

It appears to me a natural inference from these considerations that the difference of rotational velocity to which the cloud zones of Jupiter are certainly due, does not result from polar and equatorial currents carrying cloud-matter to regions where the movement of rotation is greater or less, but from upward and downward motions within the Jovian and Saturnian atmospheres. Or else, (though the assumption is not incompatible with the hypothesis of upward and downward currents), we may assume that some cause resembling that which occasions the solar spot zones is at work to produce cloud-zones on Saturn and Jupiter. We cannot proceed much farther in this direction until we have more definite information. But it suffices for our purpose to notice that we have in upward and downward currents, combined probably with some resemblance in condition to the sun, a reasonable explanation of the general features of the cloud-zones of the giant planets.

Only, it is necessary to notice that such views require the atmospheric envelopes of these planets to be exceedingly deep, and the upward and downward motions taking place within them exceedingly rapid. The upward motions would, in fact, appear, according to this view, to resemble rather the uprush of vapours during volcanic eruptions than any ordinary forms of vertical currents. The downward motions need not necessarily be so rapid; they might, and probably would, be merely the quiet return of the vapourous masses which had been propelled upwards, by which they resumed the level due to their density.

It appears to me that the results of telescopic scrutiny correspond well with these requirements. It is impossible to observe the belts of Jupiter with adequate telescopic power without being led to the conviction that the atmosphere in which these belts exist is exceedingly deep. It is impossible to convey by any description, and not easy to indicate even by pictorial illustration, the force of the telescopic evidence. It may be remarked, however, that the shapes and changes of shape of the cloud masses show that they have been generated in a deep atmosphere, and therein exposed to modifying influences. There is also one

piece of evidence on this point the significance of which is unmistakable. Scarcely ever, *if* ever, is any feature stable, even for a few successive rotations. It is certain that we scarcely ever, if ever, see the real surface of Jupiter or Saturn. I doubt myself whether those features whose successive returns have been observed for the determination of the rotation-rates of these planets are other than atmospheric phenomena. They appear to resemble rather the solar spots, whose motion may indeed suffice for the determination of the general rotation-rate, but cannot possibly be regarded as the motion of some object on the real surface of the solar orb (if he have any). However this be, it is certain that if any part of the real surface of Jupiter has *ever* been seen, the whole of that surface is usually concealed from view. Even the dark zones are not zones of his real surface, though they unquestionably underlie the bright cloud zones. Moreover, signs of violent action such as I have suggested have been by no means wanting; since not only do the features of the Jovian belts change rapidly in shape, but special details are sometimes seen,—as round white spots lasting but for a short period, &c.,—which can be readily explained as due to an uprush of vapour, and, in my opinion, can no otherwise be so satisfactorily accounted for.

But so soon as we admit the probability that the atmospheres of Jupiter and Saturn are very deep, we are led to a line of reasoning which seems demonstrative of the fact that the condition of these planets is utterly unlike that of the earth. A deep atmosphere, subjected to the strong gravitating energy of either planet, would acquire at ordinary temperatures a density altogether incompatible with the existence of any resemblance to our own atmosphere in any part of its extent. In fact, if we only assume that the atmosphere of Jupiter has a depth of 50 miles below the cloud layers, we deduce at the surface of Jupiter a density incompatible with the gaseous state, a density exceeding many times that of our heaviest metals! It is difficult to suppose that that atmosphere, the changes in which are so manifest at our great distance from the giant planets, has a less depth than this would imply; and yet the assumption that it has such a depth leads directly to the conclusion that (if at ordinary temperature) it increases in density till liquefaction is attained, and that if Jupiter has any liquid surface at all, such surface is due to the pressure of the Jovian atmosphere producing liquefaction. There would, however, be more probably continuity of change

from the vapourous to the liquid condition. I do not say that this is the actual state of matters; on the contrary, I shall endeavour to show that the real condition of the Saturnian and Jovian atmospheres is altogether different. But this is the conclusion to which we are led if we assume that these atmospheres have such a depth as telescopic observation indicates, and exist at ordinary temperatures.

But if we assume, as we must on the assumption of ordinary temperature, that, at a comparatively moderate depth below the apparent limits of the globes of Saturn and Jupiter, the liquid condition is attained by mere vastness of atmospheric pressure, or else exists simply because the atmosphere is not so deep as we have been supposing, we have, in the small mean densities of Jupiter and Saturn, a problem of no ordinary difficulty. . We cannot possibly believe (in the presence of the results experimentally shown to follow from applying great pressure to solid materials) that there can be cavernous openings in the interior of these vast globes. Under enormous pressure, solids, even such solids as iron, gold, and platinum, are perfectly plastic. They *run* like fluids. And no pressure ever yet applied experimentally is for a moment comparable with the pressure which must exist at very moderate depths below the surface of Jupiter and Saturn. It is as incredible that cavernous openings or great hollow interiors exist in the solid globes of Saturn and Jupiter as it would be that cavernous openings should exist in the depths of ocean, and with no other walls but the water itself. If the globes of Saturn and Jupiter are in the main solid or liquid, they are solid or liquid throughout, without gaps or interstices. Moreover, they are subject to a pressure constantly increasing from surface to centre, and enormously greater than that at the earth's centre, at but a relatively short distance from the surface. In Jupiter especially, the pressure must increase with great rapidity, on account of the greatness of his attractive power; for gravity at his surface exceeds gravity at the earth's surface fully $2\frac{1}{2}$ times. And again, in Jupiter's enormous globe, a depth such as that which separates the centre of the earth from her surface is relatively insignificant, being less than a tenth part of the distance separating his centre from his surface, on our present assumptions. That a solid or liquid globe, subjected to such enormous pressures throughout by far the greater part of its volume, should have a mean density less than a fourth of the earth's, as in Jupiter's case, or less than a seventh of the earth's, as in Saturn's, must assuredly be regarded as a most surprising

circumstance. Of what materials should such a globe be composed? Are there any materials whatever, of such small density, which can be supposed to exist in such relatively enormous quantities as to constitute nearly the whole of the mass of Jupiter or Saturn? It would be absurd to regard as a reasonable hypothesis, *now*, either the theory of Whewell, that Jupiter consists mainly of water, or the alternative suggestion of Brewster, that the substance of the giant planets may be of the nature of pumice-stone. Such theories as these are the really startling theories of science; they are really fanciful, because based on no known physical facts. They were unscientific even when they were propounded; but they are doubly unscientific now that spectroscopic analysis has rendered it probable that the elements constituting the great orbs of the universe are in the main the same that we are familiar with on earth, and proportioned similarly as to relative quantity. If our sun were to solidify, there would be in his globe our familiar iron, gold, copper, calcium, sodium, and other metallic elements, combined with those other elements which we recognise as the chief constituents of the earth's globe. We cannot refuse to admit the probability that what is true of the small earth and the gigantic sun, as well as of his giant brothers, the stars, can scarcely fail to be the case with the intermediate order of bodies to which Saturn and Jupiter belong. But we cannot admit this inference, probable though it is, if we adhere to our assumption that the conditions of temperature in Saturn and Jupiter resemble those in our earth. We should certainly find these planets, in that case, as dense as our earth, or rather (considering the much larger pressures to which the greater part of their mass would be subjected) we should find their mean density very much greater. Since, on the contrary, they are of very small mean density compared with the earth, we are driven from the assumption that their physical condition resembles that of the earth.

It is natural, under these circumstances, to inquire whether, since the earth gives us no satisfactory explanation of the condition of Jupiter and Saturn, we may not find in the sun some suggestions towards an explanation. It would have been natural, indeed, to have turned first to the sun, because of the much greater similarity of condition already mentioned as existing between the giant planets and the sun. But I preferred to consider first the less obvious and probable line of argument, partly to dispose of it, and partly because, unlikely and even unreasonable as it is, it is

the one which has hitherto been nearly always followed. Very few have thought of explaining Jovian and Saturnian phenomena by a reference to solar phenomena; very many, including believers as well as disbelievers in the habitability of Saturn and Jupiter, have endeavoured to force the observed phenomena into accordance with the familiar phenomena of our own earth.

We notice at once that the same low density being recognised in the case of the sun as we have been discussing in the case of the giant planets, any explanation which presents itself in the sun's case is available, at least to be tested by whatever evidence may exist.

Now we can have no hesitation in ascribing the sun's small mean density to the great temperature of his whole globe. This temperature operates against those effects of pressure which, in his case, would operate far more markedly than in the case of the giant planets. Many elements, which in his interior would be solidified or liquefied by the great pressures to which they are subjected, remain gaseous, and others which do not remain gaseous yet exist at a density far lower than that which they would have if at ordinary temperatures. We may say of the sun as a whole that its globe is expanded, and so reduced in density by excess of heat. We are not able to explain precisely how his various elementary components are disposed throughout his globe in consequence of the temperatures and pressures at which they severally exist. In fact, everything in the sun is so unlike what we are familiar with, that we cannot apply directly any of the known laws of physics to the interpretation of solar phenomena. But we remain, nevertheless, satisfied that the main reason why gravity has not its will throughout the solar orb, compressing the substance of that orb until a high mean density results, is that the great heat of the sun opposes the process of contraction which would operate at once if gravity were left unresisted.

The inference is that the orbs of Jupiter and Saturn are in like manner intensely heated, though, it need hardly be said, by no means to the same degree. Nor, in fact, can it be necessary, to maintain the mean densities of Saturn and Jupiter at their present low amount, that anything like the same degree of heat should exist in their case as in the sun's. For the pressures due to gravity are far less even near the surface of these planets, and the distance from surface to centre, on which the amount of the increase of pressure beneath the surface depends, is very much less. But that the temperature of Saturn and Jupiter greatly exceeds that

which exists in the case of our own earth, seems to be the explanation to which we are driven by the facts of the case, and, from what we know of the sun, may be regarded as a sufficient explanation.

It remains to be seen, however, whether this explanation is supported or negatived by other facts. If it be the correct explanation, moreover, not only should it be in accordance with the facts hitherto considered, but it should indicate the existence of other relations than those which have led us to it. In other words, on carefully considering this theory, consequences should appear to follow from it which correspond with the actual results of observation.

Let us begin, however, by considering the arguments which at a first view seem to oppose the theory that the globes of Saturn and Jupiter are intensely heated.

It is manifest that the heat required by the theory is such that the substance of the real globes of Saturn and Jupiter must be self-luminous, and perhaps to a high degree. Now it is certain that the discs of these planets, as we see them, do not owe their light chiefly to inherent luminosity. This is shown, not only by what I shall mention presently as to the quantity of light received from these planets, but also by a circumstance which deserves more careful attention than it has yet, I think, received. Not only does the disc grow darker near the edge,* as it would if the light were reflected light, but both Jupiter and Saturn, when near their quadratures, show a marked defalcation of light on the side where lies the terminator of the really gibbous disc. Again, we see that the satellites of Jupiter disappear in the shadow of their primary, which would not be the case if the planet shone with a very strong inherent light. An even clearer argument at a first view is found in the circumstance that the shadows of the satellites look black, as though the *whole* of Jupiter's light were reflected sunlight.

None of these circumstances, however, suffices to prove that the disc of Jupiter does not possess some degree, and even it may well be a considerable degree, of inherent luminosity. It is manifest that the vanishing of the satellites in Jupiter's shadow would only imply that his disc does not glow with an intense lustre of its own, a fact which is otherwise obvious. The darkening at the edge of the

* To ordinary vision, the reverse appears the case; but the reason of this is that the dark sky on which, as on a background, the disc is seen, causes the edge of the disc to look dark by an effect of contrast. Mr. Browning tested the matter two or three years ago with a graduated darkening-glass, and found that, as had been theoretically inferred from the aspect of the satellites on different parts of the disc, the edge is darker than the middle.

disc does not show that the light is *in the main* reflected, though it proves that a considerable share of the planet's light is reflected. The apparent blackness of the satellites' shadows would be a strong argument if we were not aware how large a part contrast may play in producing such a phenomenon. The spots on the sun look black at their nucleus by contrast with the light of the photosphere, but so does the glowing lime of the oxyhydrogen light look black against the sun's disc. And a like test is available to show that the blackness of the shadows of Jupiter's satellites may be apparent only. For the satellites themselves ordinarily look dark in transit, and the fourth satellite looks absolutely black. Now this fourth satellite is certainly not black; in fact, it shines on the background of the sky with a sufficiently bright light. We have, therefore, no evidence that the inherent luminosity of Jupiter may not be equivalent to the light which the fourth satellite reflects.

Now it is to be noticed that the light of matter glowing with intensity of heat is not necessarily intense. The red of burning coals, for example, or of hot metal is not so bright as many imagine. It is not so bright as a red object in ordinary sunlight. The very best imitation of a fire of glowing coals I ever remember to have seen was produced by a piece of rather dark red cloth, accidentally placed in a grate in an ill-lighted room (day-light). Bright red cloth in sunlight does not produce nearly so good an illusion; and it is perhaps hardly necessary to remark that the ordinary imitations of a coal-fire in theatrical representations fail in consequence of their being very much too bright.

In my opinion, there is nothing to prevent us from assuming that the darker belts of Jupiter owe the ruddiness of their colour to the glow of a red-hot interior, and this extends to the case of Saturn, whose dark belts bear a tolerably close resemblance to those of Jupiter in their general tint. I would not have it thought, however, that I limit the inherent heat of Jupiter to ordinary red heat. I simply infer that the portion of the inherent light which makes its way through the cloud-laden envelope of the dark belts corresponds to that of red-hot matter. I should regard the brighter belts, notwithstanding their greater brightness, as those whence less inherent luminosity proceeds, the greater part of their light being, I conceive, reflected from clouds of purest white, concealing the glowing surface beneath. It is to be carefully noted, however, that we have no reason for believing that any part of the cloud-belts of either planet is absolutely continuous. Openings

two or three hundred miles across would be quite invisible from the earth with the best telescopes and the most piercing eye-sight. In fact, the apparent area of such openings in Jupiter's atmosphere would be less than the fiftieth part of the least of Jupiter's satellites.

All that we might expect, I think, as an effect of the high temperature which our theory requires in the giant planets, is that these globes should shine with a light notably brighter than globes of equal size, similarly placed, and constituted of some such substance as the whiter kinds of sandstone. Now this is certainly the case. The lowest estimates of the brightness of Jupiter and Saturn assign to these orbs regarded as absolutely opaque a reflective power more than twice as great as that of white sandstone. Jupiter's brightness, as a whole, is not far inferior to that which he would have if his whole surface were of the whiteness of driven snow. When we remember how far his globe is from being white, its actual whiteness as a whole resulting from the combination of several colours, we see that, according to this the lowest estimate, he must possess some inherent lustre. But other estimates have placed his brightness far higher. And it is a notable circumstance that Dr. De la Rue, in photographing Jupiter and our moon under the same circumstances (atmospheric and otherwise), found that the actinic power of the moon is to Jupiter's only as 6 to 5, or 6 to 4, whereas if the two bodies both shone only by reflecting solar light, and possessed equal reflective powers, the moon should have nearly 27 times the actinic power of Jupiter, since Jupiter is 5½ times further from the sun than the moon is. "On December 7, 1857, Jupiter was photographed in 5 seconds, and Saturn in 60, and on another occasion the moon and Saturn were photographed in 15 seconds, just after an occultation of the planet." Jupiter should exceed Saturn about four times if both shone by reflecting solar light, and Saturn would appear, from the observation of December 7, 1857, to be of a brightness inferior to that due to its greater distance, while the other observation would imply the reverse. It seems manifest that photographic results would require a careful comparison, not only *inter se* but with some standard, to lead to satisfactory conclusions.

We may place greater reliance on direct photometric estimates; and, as it seems to me, the following results by Zöllner, when carefully studied, indicate that the condition of the outer planets differs essentially from that of the earth, Mars, Moon, and probably Venus and Mercury. He found

the light of the following planets, regarded as light-reflecting bodies, indicated the tabulated reflecting powers :—

Moon	0·1736
Mars	0·2672
Jupiter	0·6238
Saturn	0·4981
Uranus	0·6400
Neptune	0·4648*

The following determinations of the reflecting powers of terrestrial substances indicate the significance of Zöllner's results :—

Snow just fallen . . .	0·783
White paper	0·700
White sandstone . . .	0·237
Clay marl	0·156
Quartz porphyry . . .	0·108
Moist soil	0·079
Dark grey syenite . . .	0·078

I have discussed at some length and in various aspects, in my "Other Worlds" and Essays on Astronomy, the evidence in favour of occasional actual change in the shape of Saturn. It seems to me that it may now be desirable to quote the *ipsissima verba* of Sir W. Herschel as to the so-called square-shouldered aspect of Saturn. It must be mentioned in the first place that no possible question can now exist as to the ordinary shape of Saturn's disc being that of an ellipse. Herschel himself was not quite sure whether the square-shouldered aspect might not have been presented even when he made those earlier measurements which seemed to indicate a truly elliptical figure. But as we now know from the measurements of Main, Bessel, and others that the ordinary figure of Saturn is elliptical, we see what interpretation can alone be placed on the observations now to be quoted, if accepted.

It is customary to assign April, 1805, as the first occasion on which Herschel perceived any departure from the elliptical figure. But he himself quotes the following observation, made 17 years earlier :—

* I may note, by the way, that, considering the enormous difficulty of determining the dimensions of the planet Neptune, it may be questioned whether the above result might not suggest that the real dimensions of Neptune are less than usually stated, and therefore the *albedo* greater than above indicated. Of course it would be extremely rash to assume this on the strength merely of the fact that Neptune has a less albedo than any of his fellows among the giant planets. Still the point is worth noting.

"August 2, 1788, 21h. 58m. 20-feet reflector; power 300.—Admitting the equatorial diameter of Saturn to lie in the direction of the ring, the planet is evidently flattened at the poles. I have often before, and again this evening, supposed the shape of Saturn not to be spheroidical (like that of Mars and Jupiter), but much flattened at the poles, and also a very little flattened at the equator; but this wants more exact observations."

The results observed in 1805 have been often quoted by myself and others. It appears, therefore, desirable to proceed to the results obtained in 1806:—

"April 16, 1806.—I examined the figure of the body of Saturn with the 7 and 10-feet telescopes, but they acted very indifferently; and, were I to judge by present appearances, I should suppose the planet to have undergone a considerable change. Should this be the case, it will then be necessary to trace out the cause of such alterations."

"April 19. 10-feet; power 300.—The polar regions are much flattened. The figure of the planet differs a little from what it appeared last year. This may be owing to the increased opening of the ring, which in four places obstructs now the view of the curvature in a higher latitude than it did last year. The equatorial regions, on the contrary, are more exposed to view than they have been for some time past."

Then follow several observations indicating a close resemblance in 1806 to the figure which the planet had presented in 1805, when the flattening was first recognised. At length we have:—

"May 9. Power 527.—The air being very clear, I see the figure of Saturn nearly the same as last year; the flattening at the poles appears at present somewhat less; the equatorial and other regions are still the same."

These observations, combined with those made in 1805, and with subsequent observations by Schröter, Kitchener, Sir John Herschel, Coolidge, the Bonds, Airy, and others, seem to leave little doubt as to the occasional apparent expansion of the planet in its temperate zones, and also as to other changes of figure sometimes limited to one hemisphere of the planet's globe.

I find it difficult to understand how these observations, made, be it observed, by experienced astronomers, can be explained as due to illusion. They accord perfectly well with the theory which I have advocated in the present essay and elsewhere. I would not indeed suggest that owing to any processes of expansion or contraction changes take

place in the real globe of Saturn, or even that his atmosphere becomes at times heaped up in particular regions; but there is nothing to prevent the occasional existence (perhaps for long periods of time) of cloud-layers at higher levels than usual in the temperate zones, or else the occasional dissipation of the higher cloud-layers in the equatorial zone.

I have described, in my "Other Worlds," an observation of the ingress of one of Jupiter's satellites on the disc, and its subsequent reappearance, *as though it had retraced its course;* and I have shown that the only conceivable explanation of this remarkable phenomenon (witnessed by three excellent observers at different stations) appears to reside in the rapid dissipation of a high cloud-layer. I have never seen any other explanation even suggested; and, for my own part, I cannot agree with those who would simply abandon all attempt at explanation.

The general conclusion to which all the evidence, as it seems to me, would appear to point, is that both Jupiter and Saturn are in a semi-sun-like condition. It is not altogether correct to say that they occupy a position midway between that of the earth and that of the sun, for, in point of fact, such a mode of expression does not admit of any definite interpretation. It is manifest, moreover, that in some respects Jupiter and Saturn are utterly unlike the sun, while in other respects they are utterly unlike the earth and her fellow terrestrial planets. They must be regarded as *sui generis;* and it must be the work of long and careful observation with the best telescopes to ascertain the nature of these orbs. This is a subject of independent research, and although some analogies suggested by our knowledge of the earth, and other analogies suggested by our knowledge of solar phenomena, may be useful as guides, yet, on the whole, the safest course will be to pursue the inquiry in an independent manner. And here I would note that there is excellent promise of new information to be derived from the systematic observation of Saturn and Jupiter in both hemispheres of the earth.* Hitherto most of the observations

* It would not, indeed, be necessary that northern and southern observatories should be engaged in the work if an observatory could be established, for researches into the physics of astronomy, in some elevated region of our possessions in British India. For, near the equator, the ecliptic at all times passes high above the horizon. It was proposed some time since, by an almost unanimous vote of the council of the Astronomical Society, that such an observatory should be erected for the purpose indicated. Whether this proposal will be carried out remains to be seen. It unfortunately was made only as an amendment on a proposition of an eminently unsatisfactory

have been made in the northern hemisphere, though even there systematic observations have been wanting. Now Jupiter for six years and Saturn for fifteen years lie to the south of the equator, and are therefore ill-placed for observation from northern stations. It may well be that changes occur having as their period the year of either planet, in which case observations made during one half only of the year of either planet cannot reveal the law or nature of such periodic changes. And in any case it must needs be unsatisfactory that trustworthy observations should be interrupted for periods so long as those I have named.

A similar consideration applies to the Saturnian rings. It seems to me to have been demonstrated that these rings consist of multitudes of discrete bodies, though whether these be solid, fluid, or vapourous remains uncertain. Observations of both sides of the ring-system are much required, however, to elucidate the whole subject of the constitution of these strange objects. Now hitherto only the northern side of the system has been satisfactorily examined; and this side is only presented towards us under conditions favourable for study during two or three successive oppositions out of the whole series of oppositions occurring during a Saturnian year. It is worthy of remark that all the chief discoveries respecting the rings have been made at these times. It cannot but be regarded as most desirable that the opportunities afforded when the ring is most open, but the southern side turned earthwards, should not be lost. If, as is supposed, the ring-system is undergoing processes of change, systematic observations at these favourable times are essential to the inquiry into their condition.

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nature, brought forward by Colonel Strange; and it is to be feared that it may not be received with such favour in Government circles as it would perhaps have obtained if not found in questionable company. Nevertheless it is in itself an excellent proposition, and, supported as it was by all the leading members of the council except one, it is difficult to imagine that Government would refuse a favourable hearing to it. Should it ever be carried out, we can scarcely doubt that the physics of astronomy would be importantly advanced, and results obtained which at present are unattainable owing to the limited range of our northern observatories.

II. ON THE RELATION BETWEEN REFRACTED AND DIFFRACTED SPECTRA.

By MUNGO PONTON, F.R.S.E.

TO all workers with the spectroscope, an accurate method of determining the relation between the indications of that instrument and the normal positions of the spectral lines in the diffracted spectrum, which correspond to their wave lengths, has long been an object of desire.

The method hitherto followed, in ascertaining the wave-length corresponding to any line found in the field of the spectroscope, is that of interpolation, by means of the formula suggested by Mr. W. Gibbs, in "Silliman's Journal," for July, 1870. Reduced to its most simple shape, this formula may be stated thus:—Let a, b, c represent the given positions of any three lines in the index of the spectroscope, and let x, y, z represent their corresponding wave-lengths. Further make $b-a=p$, $c-b=q$, and $c-a=r$. Then, according to the formula of Mr. Gibbs, we have—

$$\frac{p}{z^2}+\frac{q}{x^2}-\frac{r}{y^2}=o$$

from which any one member may be found if the rest are given.

This formula, however, is applicable only when the three lines are very near each other—a proximity not always attainable. Moreover, it will be found that, even when the three lines are near, the result is much less approximately correct in some parts of the spectrum than in others.

The truth is, that the formula is absolutely correct only when thrown into the following more general shape—

$$\frac{p}{z^\epsilon}+\frac{q}{x^\epsilon}-\frac{r}{y^\epsilon}=o$$

where the exponent ϵ is variable, having diverse values in different parts of the spectrum.

The question thus arises, whether it may be possible to ascertain the law, according to which this exponent varies, with sufficient accuracy to render the formula available, not only for determining unknown wave-lengths, but also for correcting those already approximately ascertained by observation. Could implicit reliance be placed on an adequate number of observations, this task would not be difficult.

At present, however, the observations are neither so numerous nor so correct as to admit of the determination of the law with perfect accuracy. It is nevertheless possible, by taking advantage of the observations as they exist, to determine the manner in which the exponent varies with such a degree of correctness as may be found available for practical purposes.

The following investigation has been based on a careful comparison of Ångström's Atlas of the Diffracted Spectrum, and his Relative Tables of Wave-lengths, with the Maps and Indices of the Refracted Spectrum, by Kirchhoff, Hofmann, Ångström, and Thalén.

To find the value of the exponent ε for any three lines, of which both the positions in the index of the spectroscope and the corresponding wave-lengths are approximately ascertained by observation, is a little troublesome; because it can be done only by the method of gradual approximation, or trial and error. It therefore becomes of importance to have the value of ε determined and tabulated for every 10° of Kirchhoff's scale; because the value of ε being known, the calculation of the wave-length from the general formula becomes easy. Such a table must proceed on the assumption that the *rate* of variation of the value of ϵ remains nearly uniform for 10° of Kirchhoff's scale; and although this assumption cannot be said to be absolutely correct for every 10°, yet it is so nearly accurate as to be practically available.

For the purpose of framing such a table, it is convenient to assume the extreme lines A, and the more refrangible H of the spectrum as constants; so that, in the formula, the quantities x, z, and r may be always the same. Then if b, the index position in the spectroscope of any line, be given, its corresponding wave-length y may be found by means of the tabular value of ε in the region where b is situated. It is further convenient to assume the value of z to be 10, and to determine the wave-lengths, in the first place, in relation to this basis, converting them afterwards into the millimetric scale of Ångström. By this method we have always $\varepsilon = \log. x_\varepsilon$, which is a facility in calculation.

The assumed position of A in Kirchhoff's scale is 404·5, and of the more refrangible H 3882·5, the difference being $3478 = r$ in the formula. Then the value of p is always $= b - 404{\cdot}5$, and that of $q = 3882{\cdot}5 - b$. The log. of the wave-length x in relation to z is 1·2863197, and the log. of that log. is 0·1093490, which is one of the constants in the calculation. The log. of r is 3·5413296, and is another

constant. The log. by which the relative wave-length y found by the calculation, is converted into the millimetric scale of Ångström is 2·5947234.

The following are the values of ε for the six principal lines of the spectrum, intervening between A and the more refrangible H, based on their positions in Kirchhoff's index, and the formulated values of their wave-lengths relatively to that of H = 10.

Index position.		Log. of wave-length.	Log. of log.	Value of ε.
B.	593·1	1·2420499	0·0941391	3·1516
C.	694·1	1·2223188	0·0871845	3·1963
D.	1002·8	1·1757690	0·0703210	3·2322
E.	1523·7	1·1269776	0·0519153	3·0057
F.	2080·0	1·0919797	0·0382145	2·6244
G.	2854·4	1·0394751	0·0168141	3·3636

From the last column it will be perceived how variable is the value of ε, and how erroneous must be any formula which is based on the assumption of its being constant—more especially if the constant be fixed so low as 2.

The annexed table of the value of ε has been calculated for every 10° of Kirchhoff's scale, for the entire space between B and H. In the space between A and B, the observations are so few and uncertain that it has been found impossible to extend the table into this region with any degree of satisfaction.

A careful examination of this table will show that, were the values of ε graphically represented, they would form not a continuous curve, but a waved line—although in certain parts the line becomes straight. From 590 to 1000 of Kirchhoff's scale, the value of ε gradually rises from 3·1516 to 3·2342; but there is a slight interruption to the continuity of the ascent at 830 of the scale, where it has attained to 3·2130. Here it remains constant for 5 terms, after which it falls slightly, becoming at 880 only 3·2040. It then ascends until, at 930, it reaches 3·2310, from which point there is a second fall, until at 960 it becomes 3·2210, whence it ascends to the maximum of 3·2342, which it reaches at 1000 of the scale.

From this maximum there is a continuous and more or less regular descent, until at 1980 of the scale the value of ε is reduced to its minimum of 2·6150. From this point there is a continuous ascent to a second maximum at 3526, where ε reaches the value of 3·4520. In this interval there are several points, at which ε remains unaltered for several successive terms of the series. From 3526 of the scale

there is again a descent, which attains its lowest limit at 3770, where ε is 2·9100. This value remains constant to 3820 of the scale, from which point there is a rapid ascent to the end of the series; but in this part of the table the values of ε are somewhat uncertain.

In some parts of the series, the differences in ascending or descending from term to term are pretty regular; while in other parts these differences vary more abruptly. Their general characteristic feature is an alternate rise and fall in their value, so that, were they represented graphically, they would exhibit a very wavy line, at intervals becoming straight.

For this table, absolute accuracy is not claimed; but it is as nearly correct as it can be made in the present imperfect state of the observations. Future more accurate observations may render it necessary to introduce into it slight modifications here and there; but it is far from probable that these will affect its main features; while it may even now be trusted in the calculation of the wave-lengths, which will be correct to the fourth place of figures in Ångström's millimetric scale.

To illustrate the method of applying the scale to this purpose, let us take an example. Suppose we wish to ascertain the wave-length corresponding to the hydrogen line, which stands at 2796·7 of Kirchhoff's scale. One advantage of taking the extreme lines of the spectrum as constants is, that it presents the general formula always in one shape, namely—

$$y^{\varepsilon} = \frac{r}{\dfrac{p}{z^{\varepsilon}} + \dfrac{q}{x^{\varepsilon}}}$$

where y is the wave-length to be found, and the other quantities are all given.

In the case of the above hydrogen line, we have—

$$\begin{array}{lll} p = 2796{\cdot}7 - 404{\cdot}5 = 2392{\cdot}2 & \text{log.} & 3{\cdot}3787975 \\ q = 3882{\cdot}5 - 2796{\cdot}7 = 1085{\cdot}8 & ,, & 3{\cdot}0357498 \\ r \ \ \text{constant} \qquad\qquad\quad 3478{\cdot}0 & ,, & 3{\cdot}5413296 \end{array}$$

The value of ε corresponding to index 2800 is 3·2890
and to index 2790 3·2790

Difference 0·0100

so that 3·2790 + 67 = 3·2857 is the tabular value of ε, corresponding to 2796·7 of index.

TABLE.

K	ε	K	ε	K	ε	K	ε	K	ε	K	ε	K	ε	K	ε
590	3·1516	990	3·2330	1360	3·0611	1730	2·8158	2100	2·6380	2470	2·9475	2840	3·3500	3490	3·4319
600		1000	342	70	568	40	068	10	460	80	570	50	614	3500	
10	526	10	270	80	527	50	2·7977	20	550	90	670	60 to	664	10	
20	556	20	200	90	487	60	886	30	640	2500	770	2900		20	324
30	606	30	131	1400	449	70	795	40	720	10	850	10 to	680	30	330
40	676	40	064	10	414	80	704	50	800	20	930	40		40	380
50	736	50	000	20	381	90	612	60	880	30	3·0030	2950	710	50	450
60	796	60	3·1935	30	350	1800	519	70	970	40	130	60	750	60	520
70	856	70	873	40	322	10	427	80	2·7070	50	240	70	780	70	440
80	910	80	814	50	298	20	337	90	150	60	340	80	790	80	350
90	951	90	758	60	280	30	249	2200	220	70	450	90		90	260
700	980	1100	704	70	270	40	164	10	310	80	550	3000	820	3600	170
10	3·2006	10	652	80		50	081	20	400	90	660	10	860	10	080
20	030	20	601	90	260	60	000	30	490	2600	780	20	880	20	3·3980
30	052	30	551	1500	230	70	2·6917	40	540	10	890	30	890	30	780
40	072	40	502	10	170	80	834	50	590	20	990	40		40	480
50	090	50	454	20	090	90	749	60	670	30	3·1070	50	950	50	130
60	105	60	407	30	000	1900	660	70	760	40	150	60 to	3·4000	60	010
70	117	70	361	40	2·9910	10	570	80	860	50	260	90		70	3·2960
80	126	80	316	50	820	20	480	90	970	60	370	3100	010	80	930
0 to	130	90	272	60	729	30	390	2300	2·8070	70	480	10	020	90	730
830		1200	229	70	636	40	310	10	140	80	600	20	045	3700	300
40	120	10	187	80	541	50	240	20	200	90	720	30 to	080	10	3·1620
50		20	146	90	445	60	190	30	270	2700	830	70		20	3·0940
60		30	106	1600	349	70	160	40	340	10	940	80	110	30	310
70	100	40	067	10	253	80	150	50	410	20	3·2060	90	160	40	2·9780
80	040	50	029	20	157	90		60	490	30	180	3200	190	50	430
90		60	3·0992	30	062	2000	160	70	600	40	290	10 to	250	60	210
900	100	70	955	40	2·8968	10	170	80	720	50	390	70		70 to	100
10	150	80	919	50	875	20	180	90	830	60	490	80	260	3820	
20	215	90	883	60	783	30	190	2400	920	70	600	90 to	270	30	200
30	310	1300	848	70	692	40	200	10	990	80	700	3370		40	500
40	290	10	813	80	602	50	210	20	2·9050	90	790	80	280	50	3·0100
50	230	20	777	90	513	60	221	30	120	2800	890	90	290	60	3·1100
60	210	30	739	1700	425	70	232	40	200	10	3·3010	3400 to	300	70	2600
70	290	40	698	10	337	80	244	50	290	20	160	3470		80	4700
80	310	50	655	20	248	90	300	60	380	30	340	80	304		

To find $\frac{p}{z^{\epsilon}}$—

log. p 3·3787975
ϵ 3·2857

$\frac{p}{z^{\epsilon}}=1·239075$ 0·0930975

to find $\frac{q}{x^{\epsilon}}$

log. of log. x 0·1093490
log. of ϵ 0·5166279

log. x^{ϵ} 4·2264610 0·6259769

$\frac{p}{z^{\epsilon}}$ 1·239075 log. q 3·0357948
log. x_{ϵ} above 4·2264610

$\frac{q}{x^{\epsilon}}=0·064466$ - - - $\bar{2}$·8093338

log. r 3·5413296

$\frac{p}{z^{\epsilon}}+\frac{q}{x^{\epsilon}}=$ 1·303541 log. 0·1151247

log. $y^{\epsilon}=3·4262049$ log. 0·5348133
log. ϵ 0·5166279

log. y = 1·0427624 0·0181854
add constant 2·5947234

$y=4339·96$ 3·6374858

According to Ångström, the value of this wave-length in the millimetric scale is . . . . . . 4340·10
According to the above it is . . . . 4339·96

Difference 0000·14

which is almost unappreciable.

In judging of this result, it must be borne in mind that the chances of errors of observation, in ascertaining the wave-length and the position of the line in the scale of the spectroscope, are about equal; so that, of the above difference, small as it is, only one-half should be set down to the wave-length. If the latter be made 4340·03, thus halving the difference, then with the exponent $\epsilon=3·2857$, the position of the line in the scale of the spectroscope would have to be made 2796·56, which differs only 0000·14 from that assigned to it by observation—a difference which is in like manner scarcely appreciable. As determined by the table, then, this hydrogen line ought to have for its index position 2796·56, and for its wave-length 4340·03.

The mode of finding the value of ε, when both the position of the line in the scale of the spectroscope and the wave-length are given, and held to be absolutely exact, differs but little from the foregoing. A certain value of ε must be assumed, and when log. y^{ε} and its log. are found as in the preceding calculation, the log. of the log. of y, as given by observation, is to be subtracted from it. If the value of ε has been accurately assumed, then the difference should be exactly equal to log. ε. If the difference be greater than this last, then the assumed value of ε must be increased; if the difference be less, then the value of ϵ must be diminished. A little experience will show the extent of the requisite increase or diminution, which varies in different regions of the spectrum.

Thus, in the foregoing case, if from the log. of log. y^{ε}	0·5348133
We subtract log. of log. of the relative wave-length corresponding to 4340·1 of the millimetric scale	0·0181911
We have	0·5166222
Which is less than log. ε by	0·0000057
Log. ε . . .	0·5166279

Thus showing that the value of ϵ should be slightly diminished. The requisite diminution is 0·0015, making the exact value of ϵ, on the assumption that the observations are absolutely correct, 3·2842.

It will hence be perceived how very sensitive is the exponent, and how considerable a variation it may undergo even with a scarcely appreciable difference of wave-length, or in the position of the line in the spectroscopic scale.

In the table (p. 31), the columns marked K contain the degrees in Kirchhoff's scale; while those marked ϵ contain the corresponding values of the variable exponent. The table is submitted in the hope that, notwithstanding any slight imperfections it may be found to contain, it may prove useful to those who are engaged in spectroscopic investigations.

III. OBSERVATIONS ON THE OPTICAL PHENOMENA OF THE ATMOSPHERE.

By S. Barber, F.M.S..

DURING the first quarter of the year 1872, many remarkable optical phenomena were observed in the neighbourhood of Liverpool; and as I have on previous occasions published remarks bearing on the prognostic value of these appearances,* it seemed advisable to collect and compare such notes as may bear upon the prediction of weather, and help to elucidate the questions that have been raised as to the origin, and varieties of halos and parhelia. The subject, certainly, is one of the highest interest, both in relation to the various crystalline forms of water and the constituent particles of the various forms of cloud. A beam of light passing through the upper atmosphere may reveal to us, by the laws of refraction, the degree of congelation of vapour floating ten or twenty miles away. The investigation of the cirrus and cirro-cumulus being admittedly of the highest importance in relation to the movements of the great atmospheric currents, it seems surprising that the optical laws and phenomena bearing on their constitution should not have received more attention at the hands of meteorologists.

Before passing on to the proper subject of this paper—viz., halos and parhelia—I may remark that the rainbow, which appears to have been so thoroughly investigated, exhibits at times more variety, both in form and colouring, than most writers seem to be aware of. These varieties will be found—like those of the halo—to have a relation to the general atmospheric conditions of the time. For example, I have frequently noticed that the tendency to an irregular outline, and the appearance of double bows, &c., are mostly connected with stormy and squally weather. A dark day in autumn, with low-flying nimbo-cumulus, and driving rain, will sometimes exhibit, in its transient gleams of sunshine, most curious and unexpected forms. On such occasions, though the outlines are more remarkable, the colouring is not so brilliant and decided as in calmer weather.

In the last "Quarterly Journal of the Meteorological Society," Mr. Scott gives notes of a double rainbow, with reversed colours; and Mr. Lecky adds an account of a

* Quarterly Journal of Science, No. 26.

triple one produced by reflection from the surface of water—seen of course in calm weather. I may add to these an observation of a bow, which was almost devoid of colouring, and divided into separate rings, four or five in number, concentric, and decreasing in width towards the centre,* so that the innermost was almost invisible. These rings were near together; and the originating cloud seemed of low altitude.

On another occasion I have observed a bow in which the outline was decidedly broken and unsymmetrical. This was also in the cumulus cloud, and in stormy weather.

I now pass on to the forms of halo and mock sun; two of the latter were seen during the winter and spring of 1872. The first was imperfect in definition, as seen from Aigburth, about 4½ miles south of Liverpool. This, though not a brilliant form, is worthy of record from the fact of its having been seen at stations widely distant from each other. It was described in the "Times," as seen at Meath, in Ireland. On comparing notes, I found that this description agreed in several points with the observation made by myself. The mock sun as it appeared near Liverpool was of an oval form, and situated vertically over the real sun. It lay at the point of contact of two arcs of halos tangent to each other. There was no cloud in the sky, only a slight haze, such as occurs after heavy dew and hoar-frost. This occurred about 9.10 on the morning of Jan. 22. There had been a slight frost during the night.

The phenomenon having been observed at several places some hundreds of miles apart, the originating crystals must have been of unusual altitude and extent. In passing, I may remark that the height of mock suns is probably very inconstant. Thus, in the cold weather of spring (1871), I was fortunate enough to take an observation of the sudden formation of a well defined ball of prismatic light, which owed its existence to the passage of a rapidly moving *cumulus* cloud. This could scarcely have been higher than 5 or 6 miles at the most. The mock sun was very similar, both in form and position, to that described by me in the "People's Magazine" (February, 1872)—and it appeared and disappeared with the cumulus cloud. The weather changed to heavy rain on the day following the mock sun of Jan. 22, 1872.

The next instance, of which I took more careful notice,

* The outer ring was about half the width of an ordinary rainbow, and the inner ones became narrower by a fixed proportion.

was visible from the Mersey, at Liverpool, from about 11.50 till 12.15 at noon, on Wednesday, April 10th. (See sketch on next page.) It seems to me to be a very unusual form. The wind was nearly N. at the time, and the weather cold. As I have ventured to maintain that halos and kindred optical phenomena generally indicate a transitional state of the temperature or of the weather, and not, as is popularly supposed, an approaching storm, I draw attention to this case as an important, though by no means isolated, confirmation of the theory. It followed many days of *wet*, and preceded about a week's *dry* weather. On referring to the sketch it will be seen that an arc of a halo attended the mock suns (which were not very brilliant), and that the chief peculiarity of the case lay in this—that the mock suns were clearly external to the halo, and at a distance of 1° 30′ or 2°. The halo showed chiefly a red colour, and the parhelia were entirely of a light prismatic blue, very distinct from that of the surrounding sky.

Exactly a week after the last mentioned phenomena were seen, a solar halo again occurred about 1 p.m., Wednesday, April 17, and after this the weather grew warmer and the sky still clearer. We shall probably not be justified in expecting a *decided* change after every appearance of these phenomena, for I have often observed that, in showery and changeable weather, two or three solar halos of varying degrees of definition will occur on successive days; but for several years I have found that, when there is anything very singular in the form or combinations of the circles, changes of a decided character often follow.

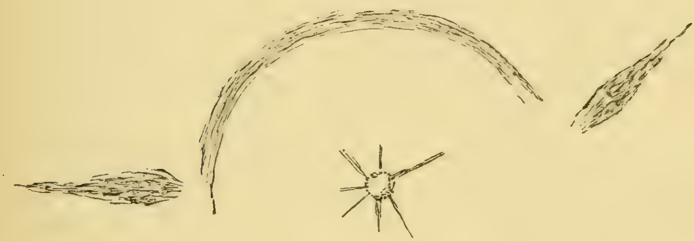
Upon examining the recorded notices of these appearances, we find that there are usually more accounts of lunar than of solar halos, yet I have no doubt, from my own observations, that more *solar* halos really occur, and that most of them are overlooked on account of the dazzling effect of the sun's rays, rendering their observations somewhat troublesome to those who have not a strong eyesight. I can safely say that in the neighbourhood of Liverpool they greatly preponderate in number. Scarcely any change, indeed, from wet to fair, as well as from fair to wet, occurs without their appearance. Two more years' observations enable me to endorse the statement concerning their prognostic value, which I published in spring, 1871.* I desire now, however, to draw attention to two peculiarities connected with the lunar halo which appear to be of very

* Nature, March 23, 1871.

evil portent. M. W. de Fonvielle, in commenting on an appearance of very rare form of lunar halo, of which I had sent an account to "Nature" (Jan. 26, 1871), makes the following remark:*—"Il est presque inutile d'ajouter que l'apparition de ce halo a été suivie, comme d'ordinaire, d'une chate de neijes abondantes." Of course this remark refers to the winter season. I am not aware, however, that any case is on record of a halo of 90° appearing in the summer time. Other similar cases of equal prognostic value have since been observed. One of the most remarkable occurred during the autumn of 1871.

This was a long horizontal band of light that proceeded in a straight line from the moon to a distance equal to the radius of an ordinary halo. At the point where this line would have intersected the halo there were two short bands of light passing through it vertically, so as to form a kind

FIG. 1.



Parhelia. April, 1872.

of cross laid horizontally in the sky. Another phenomenon, which, I imagine, has a kindred origin, was observed by me, near Liverpool, on the Sunday before Easter, 1872. On this occasion the cross was formed at the moon itself, and the bands were only two or three degrees in length, at right angles to each other. After these appearances I took particular note of the weather. In a day or two, very heavy rain set in, and continued for a long period. Students of Meteorology will long remember the summer of 1872, and it is remarkable that it should have begun with so many and such unusual optical phenomena. This fact, however, is particularly noteworthy—that after the great electrical disturbance set in, and till the termination of the storms, not a single instance of a well defined halo or any

* Comptes Rendus, Fevrier 27, 1871.

cognate optical appearance was seen; and equally remarkable was the absence during the same time of the cirrus and cirro-cumulus forms of cloud. It would appear that the amount of electricity diffused through the atmosphere had some influence on the congelation of vapour, or that the forms of cloud in question are in some way dependent on an electrical condition, or a relation of the upper to the lower stratum of vapour. During this season the electric cumulus, of various forms and shapes, was the prevailing cloud form.

Professor Poey, in his recently published remarks on cloud classification, points out that, during the most pronounced rain seasons, the two kinds of "pallium,"—viz., the high ice-pallium and the low mist-pallium—are separated by a neutral region of the atmosphere, and that the pallio-cirrus is then in a state of negative electrical excitement, in common with the air at the surface of the ground; while the pallio-cumulus and the rain that issues from it are in positive electrical excitement; and that electrical discharges continually take place between the two concomitantly with the pouring down of *unelectrified* rain from the lower stratum upon the earth.

These results are singular and interesting, though it is to be regretted, perhaps, that Prof. Poey has not given some details of the means by which they were obtained. It has seemed strange to me that the invention of atmospheric electrometers and electroscopes, in combination with the Captive balloon, has not been put to some practical use in the prediction of weather. By placing several of these instruments at various altitudes, and synchronously noting their indications, further important results may perhaps be obtained.

On referring to Buchan's "Handy Book of Meteorology," I find a case of a halo intersected by twelve rectilinear bands of light proceeding from the sun. No remark is made on the origin of the bands; but experiment seems to show that we must attribute these to reflection, and look for the cause in the configuration of the terminating facets of the suspended crystals. This theory is corroborated by the fact that these bands of light are achromatic, which would not be the case if they were caused by refraction.

A halo broken in its outline, and caused by well-defined bands of linear cirrus, is, I believe, a more certain sign of rain than a continuous circle. Another form that I have noticed before bad weather is a fleecy and irregular circle, wider in some parts than in others; it is of a very evanescent

character, and has but little colouring. This form, which is very uncommon, appears sometimes when the sky is almost clear.

A few notes on recent appearances of parhelia, in relation to the attendant halos, may not be inappropriate. The sketch given of *blue* parhelia accompanying a *red* halo, and clearly external to it, illustrates, I think, a very uncommon case. It will also be seen that, while most mock suns have an elliptical or oval outline, and the major axis in a line with the *circumference* of the halo, the major axis of this lay in a line with the *radius* of the halo, and the balls of light were not symmetrically placed.

Cases of parhelia without any halo are very rare. An instance may be found recorded in "Nature," of an oval mock sun entirely within the attendant circle, and situated vertically above the real sun. Most of the cases of parhelia noted by me have been in a horizontal line, not a vertical one; and those situated above the sun have been very deficient both in colouring and definition of form.

One remarkable feature of these appearances is this, that when the parhelia are seen in very perfect form, the halo that accompanies them appears only to show one colour, and that generally red I believe; whereas the ordinary solar halo has blue for the most part predominant.

I now pass on to consider the various media, or conditions of the atmosphere that give rise to these optical phenomena. The form of cloud denominated by Howard cirro-stratus is, as he himself remarked, the most frequent originator both of lunar and solar halos; but ordinary cirrus also often produces them in an imperfect form. The normal or "curling" type of cirrus seldom shows prismatic tints, though we know it to be strictly an ice-cloud; nor does the form of cumulus which we may call "snow cumulus." This seems strange when we consider that the halo is simply a result of ice refraction; but it may be explained upon the hypothesis that, in these two forms of clouds, the particles are so massed together, and the prisms and crystals so overlaid one upon another, that the refracted rays are again combined and a white line produced, as may be seen on a close inspection of a fresh snow-flake.

Taking, therefore, the various forms of cloud, we find that halos are usually restricted to the following:—

1. Cirro-stratus, or as it is sometimes called linear cirrus.

2. Ordinary cirrus. On this form I may remark, that it is when somewhat thin and transparent that the halo appears.

It would be well, perhaps, to make distinction between the kind of cirrus here alluded to and the whiter and denser variety, which approaches in character to cirro-cumulus. This variety is evidently a different composition, from the fact of chromatic phenomena being absent. The surface is whiter and less marked, and the outline more distinct than in the halo-producing variety.

3. "Club" cirrus. I thus denominate the long narrow line of cloud which proceeds from a kind of tuft partaking of the nature of cirrus and cirro-cumulus.

These are the commonest forms of *cloud* that produce halo. We now take plain skies. I have observed the phenomenon under the following conditions:—

1. A light greyish blue. This is very unusual.

2. A deep opaque blue. On this occasion the halo was entirely red, as when the parhelia appeared.

3. High thin mist (probably floating crystals). This is not uncommon in frosty weather. There is a general law with regard to these phenomena which may be stated here, viz., that, "the clearer the sky the more perfect is the colouring." This I have verified by many cases. It may also be added that the more defined the clouds of the upper stratum, the less likely are halos to be seen.

IV. RECENT CHANGES IN BRITISH ARTILLERY MATÉRIEL.

By S. P. Oliver, Capt. R.A.

I. EARLY this year experiments have been carried out both at Shoeburyness and on board H.M.S. *Excellent* with some modified 24-pounder Hale's rockets to test their range, accuracy, and incendiary power in comparison with the ordinary service rockets, Mark III.

The modified rockets have the internal form of the cone in the composition altered and a modified tail piece. These alterations were expected to have the effect of greatly increasing the velocity, duration, and rapidity of rotation, ensuring greater range, greater accuracy, and less tendency to puff. The modified rockets were found to be (with one exception) all steady in flight, whilst all the service rockets puffed more or less. Those fired from the *Excellent's* cutter with Fisher's rocket apparatus gave good results, with the

exception of two, which burst prematurely in the air and blew their heads off. Their incendiary power was tested as follows:—Their heads were filled with Carcass (Valenciennes) composition. Four rockets of each nature were firmly fixed in a hole bored in a balk of timber, their heads protruding and butting against another balk covered with inflammable material. One of the rockets, after the composition had burnt out, burst explosively, and blew the head to pieces before the Carcass composition could be ignited. Mud was heaped upon the heads of the others while the composition was burning and the flame forced its way through; water was also poured upon the heads without extinguishing the flame.

Capt. Boys, R.N., considers that the incendiary powers of the new modified rockets over those of the service are considerably increased—that their range is rather better. The deflection of both rockets is equally variable, the maximum of service rockets being 100 yards, and that of the experimental rocket 80 yards on either side of the target. The Lords Commissioners of the Admiralty therefore consider the modified rockets to be preferable for naval service; the defect of premature explosion can easily be remedied.

II. The Electro-Ballistic experiments under Capt. Noble have been continued, and tables have been arranged showing the comparative probable rectangles of various ordnance, and the greatest velocities obtainable by different guns. The highest mean muzzle velocities obtained from the 9-pounder and 16-pounder rifled muzzle-loading guns, using service common shells and service R. L. G. powder, were found to be respectively 1562 and 1466 feet per second.

III. The report on the traversing arrangements are satisfactory. After practice two men can easily traverse a 9-inch gun from right to left (arc about 110°) in 45 seconds. Some of the 9-inch platforms are being tried with traversing gear on the spur- and mitre-wheel principle.

With regard to the recoil of guns, the Elswick compressors have been found to work most satisfactorily, and are to be retained where already fitted to 7 and 9-inch gun platforms.

IV. Important experiments and results have been carried out and obtained by the Committee on gunpowder and other explosives, especially as regards battery charges.

In determining the battery charge for any gun, the proper course to be followed, in the opinion of the Committee, is as follows:—To increase the charge gradually until distinct wave pressures are exhibited; the highest charge which can

be employed without these local pressures appearing should then be accepted as the battering charge. Experience has clearly demonstrated that, by limiting the charge in accordance with this rule, the maximum useful effect is attained without any risk of undue pressures in the gun; and that if the charge thus fixed is exceeded, a portion of the powder is wasted, and the gun rendered liable to undue local pressures. The charge of 85 lbs. of pebble powder exceeds that which the above-mentioned rule would give for this gun, but to no dangerous extent, although the maximum useful effect of the charge is not obtained.

In determining the battering charge, a comparison between the power of the gun with a calibre of 11 and 12 inches came incidentally under the notice of the Committee; the results of the experiments clearly demonstrated that a gun of 145 inches length of bore is more powerful as a battering weapon with a 12-inch calibre than with one of 11-inch, and this is still more evident when it is considered that with a 12-inch calibre the gun would probably consume 95 lbs. of powder with as good useful effect per lb. of powder, and with no greater pressure per square inch than it does 85 lbs. of powder with an 11-inch calibre.

The detail has been published of various experiments with 35-ton rifled muzzle-loading gun, No. 1. Shot, 700 lbs.

The pressures were determined by crushers fitted by means of a copper cup at the bottom of the bore (A), by a screw gauge inserted instead of the vent plug (B), and by a gauge screwed into the base of the projectile (C). After round 8 the gun was fired by an electric tube placed in the cartridge at 12 inches from the bottom, the wires passing through a groove in the shot to the muzzle.

The following are a few of the points elicited by these experiments, which appear of the greatest general interest:—

1. If the powder be burned uniformly in the gun, without any indication of wave action, the pressure increases with the increase of charge, at first rapidly, but after 20 tons on the square inch has been exceeded, then very slowly. In the whole course of the Committee's experiments a uniform pressure by crusher gauge of 30 tons in the powder chamber has never been attained; this fact appears strongly to corroborate the experiments carried out by Captain A. Noble, at Elswick, on the pressure produced by ignited powder in closed vessels, which indicated that the maximum pressure produced by ignited powder in a perfectly closed space is somewhat less than 40 tons to the square inch.

2. When a charge of any description of powder is increased beyond a certain limit, wave or local pressures are set up which strain the gun unduly, without affording an equivalent of useful effect on the projectile.

3. Provided the battering charge is not exceeded, the pressure in the gun increases steadily with the increment in weight of the projectile up to a certain point; beyond this point no material increase of pressure can be obtained by increasing the weight of the projectile.

4. Proof of pebble powder of Waltham Abbey and trade manufacture in accordance with specification.

This proof has been carried on with the 8-inch smooth-bored gun prepared for the purpose. The Committee consider the results to be on the whole very satisfactory, and express their unanimous opinion that the velocity and pressure test of powder for heavy guns should never be suspended, as they are satisfied it is the only proof that will ensure the supply of powder good and uniform in quality.

Considerable difficulties have been experienced, both at Waltham Abbey and by the merchants, in keeping the powder up to the specification; this might be reasonably expected in the production of a new article. The Committee consider themselves justified in saying that the progress made is encouraging, most of the difficulties having been overcome. Valuable knowledge is daily acquired, and light thrown on many points by the system of proof adopted on their recommendation.

V. Another explosive picric powder consists of only two ingredients, saltpetre and picrate of ammonia, the ingredients being incorporated in the same way as those of gunpowder.

The perfect stability of the two ingredients, *per se*, even if exposed to degrees of heat very far beyond the extremes of tropical temperatures, has long been fully established.

Picric powder is certainly not *more* susceptible of explosion by friction or percussion than gunpowder.

Its exploding point has been compared in several ways with that of gunpowder. In some instances the picric powder exploded at a somewhat lower temperature, or a little more readily, than gunpowder; in others the reverse was the case. The two substances may, therefore, be considered to have about the same exploding point.

Samples of picric powder, prepared early in 1870, which have been exposed to light and preserved at ordinary atmospheric temperatures, are perfectly unchanged.

Samples have been exposed for several days successively to 212° Fahrenheit without sustaining any change.

Samples have also been heated for several hours daily, during several days, to a temperature ranging from 300° to 320° Fahrenheit. The picrate of ammonia was slowly volatilised from the powder by this treatment, just as the sulphur would be from gunpowder. In other respects the powder was unchanged.

A sample of picric powder, which had not been submitted to pressure, was exposed in an atmosphere saturated with moisture for 18 days, when it had absorbed 14 per cent of water; it was then exposed to the open air at the ordinary temperature (September, 1870), and was found to have parted with the whole of the water absorbed.

At the end of 20 days another sample was exposed to the damp atmosphere until it had absorbed 5 per cent of water in 6 days; upon subsequent exposure day and night to the air it returned to its original weight in 8 days. Its exposure to the open air day and night was afterwards continued for 40 days, in the months of September and October, and its weight ascertained early each morning. The maximum increase in weight during the experiment was 6 per cent. On the 40th day, as on several other days during the experiment, it had returned to its original weight, and its properties were unchanged.

Not the slightest indication has been obtained, in very searching experiments, that picric powder is in any respect more prone to change than gunpowder.

Further trial of picric powder will be combined with the trial of gun-cotton pulp as a bursting charge for shells.

In the opinion of the Director of Artillery, picric powder has the disadvantages of gunpowder as regards danger, and, on the other hand, is not equal to gun-cotton as to power or safety.

The quantities of picric powder already tried have been small, and it has not as yet developed qualities to lead to the conclusion that it will either supersede gunpowder or gun-cotton.

Considering, therefore, that as the qualities of gunpowder are known, and those of gun-cotton are being developed, he thinks it would be premature to introduce picric powder into the service at present, but that it had better remain in an experimental stage until its actual properties and comparative safety are more fully known.

But the Director of Naval Ordnance considers it desirable that further experiments should be carried out with picric

powder, as it may prove a valuable explosive for shells and the Harvey and outrigger torpedoes, where the amount of charge is limited.

VI. The following experiments have been made by the Special Committee on Explosives upon an explosive substance termed "Pyrolithe," which is being manufactured in the town of Middlesborough, to determine whether it is of such a character as to bring it within the meaning of section 6 of Act 23 and 24 Vict., c. 139:—

1. 1 lb. tin gunpowder canister (obtained from Messrs. Curtis and Harvey) filled with pyrolithe taken from No. 1 cask.

The canister was laid upon the ground, and the material ignited by means of a piece of Bickford fuze placed in a hole in the side.

The contents burnt with considerable violence, but without exploding; the solder of the case melted almost immediately, and the pyrolithe burnt fiercely from the apertures thus caused until consumed.

2. 1 lb. tin gunpowder canister filled with pyrolithe taken from No. 2 cask.

The nose of the canister was placed on the ground to prevent it from being immediately blown off, and a small piece of iron was placed on the canister to weight it, and to represent the condition of one package in a box, or under others.

The material was ignited (as in the previous case), and exploded with a dull but decided report, and with sufficient force to project the canister (which was ripped open) to a distance of fourteen yards.

3. 1 lb. tin gunpowder canister filled with pyrolithe from the cask *not marked*.

The results were the same as those obtained in No. 1 experiment.

4. 1 lb. tin gunpowder canister filled with pyrolithe from the cask *not marked*.

This canister was placed in a fire of wood and coals laid in a brazier. In about four minutes the pyrolithe ignited and burnt fiercely, but without exploding. The solder of canister was melted.

5. Small wooden keg (with a capacity to contain 5 lbs. of gunpowder) filled with pyrolithe from No. 1 cask and headed up.

The keg was ignited by means of a piece of Bickford fuze

introduced through a hole in the side. On the pyrolithe taking fire the head of the keg was blown out with a slight report, and the material then burnt rapidly away.

6. Small wooden keg filled with pyrolithe from No. 2 cask.

In order to prevent the head of the keg being too readily blown out, and to represent the condition of barrels stacked one on the other, a weight of about 50 lbs. of iron was placed on the head.

The pyrolithe on being ignited blew the head and the weight off with a decided report, and having thus found a vent burnt rapidly away.

7. Quarter casks, 1, 2, and 3 (containing about 20 lbs. of pyrolithe in each) headed up, placed in original packing case, and then covered over with shavings and wood.

In a few minutes after the shavings and wood were ignited, one cask caught and burnt violently; in about 15 seconds more the second cask caught and the flame became more violent; and in about 30 seconds the third cask caught, and caused an almost explosive burst of flame; the whole then burnt with considerable fierceness until consumed.

On full consideration of the results the Committee are of opinion that under conditions which might arise in connection with the manufacture, transport, or storage of pyrolithe, its ignition would be followed by a more or less violent explosion, and consequently the character of this substance comes within the meaning of the Act quoted.

Samples of pyrolithe from each of the three casks were forwarded to the Chemist to War Department for analysis, and the following are the results obtained:—

Description of Sample.	Soluble matter consisting of nitrates, carbonate of soda, and sulphate of soda.	Composition. Sulphur.	Saw-dust.	Percentage of moisture.
No. 1, P. $\frac{5}{\text{S.M.}}$	70·94	16·00	13·06	9·01
No. 2, P. $\frac{\text{8 C.}}{\text{P.}}$	73·08	16·12	10·80	8·88
No 3, not marked	69·89	16·36	13·75	8·88
Old sample which had been in Major Majendie's possession for some time.	70·94	16·14	12·92	3·07

VII. Complaints, it appears, are frequently made with regard to the irregularities in burning of Boxer wood time-fuzes, especially in mountain batteries, under varying atmospheric pressure above the sea-level.

The whole subject of the influence of local altitude on the burning of time-fuzes has been carefully investigated both practically and theoretically,—

Practically by Quartermaster Mitchell, in India, in 1849, at different altitudes, viz.:—

St. Thomas's Mount . . .	Sea level.
Bangalore	3000 feet.
Kotagheny	6500 ,,
Cotacamund	7300 ,,

Theoretically by Professor Frankland, in 1860, and subsequently, practically, by the French Academy of Science, at altitudes, viz.:—

Oucby	1246 feet.
St. Pierre	5380 ,,
Chenallettis	9481 ,,

The agreement in these different and distinct observations was most remarkable, and from them was deduced the following practical rule:—

That the time of burning of a fuze increases in the ratio of 0·0011 of its value for each diminution of 0·0394 inch of barometrical pressure; or, in other words, of about 0·03 of its value for each variation of 1 inch pressure. Atmospheric pressure diminishes almost uniformly at the rate of 1 inch for every 1000 feet of altitude; hence the time of burning of a time-fuze increases 0·03 of its value for each increase of 1000 feet of altitude, or 0·003 of its value for each variation of 100 feet of altitude.

Thus if a 9-seconds fuze burns exactly 9 seconds at the sea level, it will burn 11·16 seconds at an altitude of 8000 feet above the sea. The times of burning of the fuzes at the sea level will, in future, be placed in the cylinders, together with a notification that "The time of burning increases nearly 3 per cent for every 1000 feet of altitude."

IX. It is most important that an efficient gas-check should be provided in order to prevent the erosion in the bores of muzzle-loading rifled guns which is caused by the scoring rush of gas over the top of the shot, more especially when firing with pebble-powders. Various tin cups and cow-hide wads, &c., have been tried, at present without success, and the service wad is discontinued as useless.

X. Colonel Inglis's muzzle derrick will be adopted for

9-inch guns and upwards when mounted in open batteries and *en barbette*.

XI. The 400-lb. shell, common 10-inch, when fired from the 18-ton gun, has been found sufficiently strong to penetrate the sides of wooden unarmoured ships without breaking up; but with regard to the 7-inch projectile from the 90-cwt. gun, the Committee come to the following conclusions:—

1. That the present experiment affords no trustworthy evidence of the relative destructive effect which would probably be produced by common shell after passing through the side of wooden and iron unarmoured vessels. It is worthy of note that after passing through the side of the wooden target, a shell, if it does not break up or explode at once, is liable to turn sideways. Under such circumstances the projectile would probably lodge, and might act as a mine in the opposite side. Exact information on this point cannot well be obtained without firing at a decked structure, or an actual vessel.

2. So far as destructive effect is concerned, the Committee are unable to form any trustworthy estimate of the comparative value of any of the projectiles fired.

The special 7-inch projectile has the advantage of an increased bursting charge, but this appears to entail a loss of strength in the shell, and, looking to the inconvenience of adopting a new pattern, the Committee, on the whole, prefer the service shell, weighing filled 115 lbs.

3. The distance at which common shell break up or explode after passing through the side of a vessel, either of wood or of iron, depends in a great measure on the nature of the resistance met with. If the shell hits on a knee, a rib, or a diagonal iron brace, it almost invariably breaks up or explodes in passing through the side; if, on the other hand, it passes fair between two ribs in a place where the resistance is confined to the wood planking or iron plating, it may not break up or explode until it has passed from 6 to 10 feet from the side.

Complete information on these points cannot, however, be obtained without practice at an actual ship.

4. That the projectiles fired from both guns were liable to break up (without bursting) in passing through either of the targets. The shells appeared to break up whether they were filled with sand or with gunpowder; in the latter case the bursting charge in several instances merely fired. The full effect of the explosion did not appear to be realised unless the shell struck on a part of the target where, owing

to increased resistance, the onward velocity was suddenly checked.

XII. The Moncrieff system of mounting guns has been tried successfully with guns up to 7 tons weight, can be applied with advantage to 9-inch guns of 12 tons, and possibly extended to guns of 18 tons and upwards.

The 9-inch carriage subjected to trial was of the improved type, known as Pattern II. It differs from the original construction adopted in the case of the 20 7-inch muzzle-loading Moncrieff carriages made for service, and from the first experimental carriage for 9-inch guns of 12 tons, in the following particulars:—

1. The carriage proper is dispensed with, and the gun rests directly on the elevators. By this arrangement, the strain which, in the carriages of original construction, was received upon the rear axletree at the moment of firing, is now conveyed through the vertical elevating bars to a grooved stool bed upon which these bars slide. By this means the blow which previously was met directly is now gradually absorbed.

2. The gun comes down into a constant position for loading, whatever elevation or depression it may have in the firing position.

3. It is simpler and more compact, although heavier, but it affords greater facility for loading and elevating.

Notwithstanding the increased cost of the Moncrieff as compared with the service carriage and platform, his system (an ordinary barbette unprotected battery always excepted) is considerably cheaper than any other, constructed of either earth or stone protected with iron shields. As compared with the cost of a casemated battery, with shield, as estimated for a 9-inch gun of 12 tons, the balance in favour of the Moncrieff system would amount to about £1800 per gun, while, as compared with that of an open battery, with shield, with and without splinter-proof cover, the saving would be respectively £450 and £667 per gun.

As regards economy and efficiency, therefore, the Committee consider the Moncrieff system compares very favourably with that of the service, especially when it is considered that, from its extensive lateral range, one gun mounted on a Moncrieff carriage may do equal work with two or more guns mounted behind shields.

The system will be found particularly well adapted for,—

(1.) Mounting guns in salients, &c., of land defences, and—

(2.) Mounting guns for subsidiary defence of existing heavy sea batteries; they allude more particularly to such works as Picklecombe, Bovisand, &c., the guns of which being essentially armour piercers, should have associated with them guns of lighter calibre for shell fire.

(3.) The defence of the great commercial harbours.

The expense of mounting a few 12-ton or possibly heavier guns on Moncrieff carriages would be considerably less than placing them behind shields or in casemates; while the increased protection afforded to the men over that of guns *en barbette* would be a matter of great importance.

With regard to the employment of the Moncrieff system for mounting guns of large calibre on sea defences, the Committee consider that it might be resorted to with advantage, but the extent of its application necessarily depends upon local and other considerations, of which they can have no cognizance.

Should it, however, be contemplated to project new works for the defence of important positions, or to supplement existing works by others of the present type, the Committee are strongly of opinion that the designs should be re-considered with a view to the employment of the Moncrieff carriage.

XIII. The manufacture of 8-inch rifled muzzle-loading howitzers for the service is being proceeded with.

1. As a howitzer, this piece will be usually employed in destroying earth works, in breeching unseen defences, or in shelling buildings, ships, &c.

In these operations the elevation will seldom exceed 20°, and, as a rule, high charges will be used, the shells being designed to burst on impact, or by means of a percussion fuze.

2. As a mortar, it will be used in bombarding magazines or other bomb-proof buildings; in dropping shells upon the decks of vessels; in dislodging troops from cover, or in destroying *matériel* behind cover, &c.

In these operations the elevation may vary from 20° to 40°, or even higher angles, and the charges from the highest to the lowest. As a rule, however, low charges will seldom be used, except in dislodging troops from under cover, and under these circumstances a time fuze will generally be found most effective.

Elongated projectiles falling at angles of 50° to 70°, or under conditions of vertical fire, will enter the ground for

several feet before a percussion fuze will have time to act; thus the effect of the explosion will be comparatively slight in a lateral direction.

The effect of shells burst in the air, over the heads of troops, or just in clearing the parapet, would be much more searching than the effect of shells which had entered the ground before exploding.

Again it is an open question which nature of fuze would be best when firing at bomb-proof structures.

It is possible that under these circumstances the wood time fuze would act percussively, but by cutting it long the shell might be given time to enter to its greatest depth for exploding; it would thus act with the greatest advantage as a "mine."

XIV. From experiments with guns fired from casemates, and behind shields at Picklecombe, Bovisand, and elsewhere it has been found—

1. That a slight difference in protrusion of muzzle of gun has an immense effect with regard to concussion and smoke, which are much lessened.

2. That the mantlets materially lessen the amount of smoke and concussion in casemate, but not sufficiently so to allow of many contiguous guns being worked at close interval when firing rapidly.

The side pieces or wings are somewhat cumbrous to move; do not allow sufficient play for bringing the mantle up to the gun when trained at an angle; and are in the way of men loading when the gun is trained at any considerable angle.

3. The solution of chloride of calcium to render the rope uninflammable answers admirably.

XV. The experiments with the 35-ton guns have been also satisfactory. Some difficulties have been experienced in loading when the recoil is less than five feet, and it is necesssary for one of the gun detachment to hold up the end of the rammer outside the work, the leverage of the stave being too great for Nos. 2 and 3 to support it within.

The shooting of the common shell of 618 lbs., with full charge of 85 lbs. of pebble powder, is better than that of the Palliser shell of 700 lbs., and battering charge of 110 lbs., which is principally due to the shearing of the front studs of the latter and consequent increase of gyration, which causes inaccuracy and a want of uniformity in range and deflection.

Meantime improvements, both in armour-plated ships and armour-piercing guns, continue to be made, and whilst at

Portsmouth the double turreted *Inflexible* is being built with 20-inch plates for her citadel at Woolwich, a sixty ton experimental gun, with calibre of 15 inches, to throw a projectile of 1100 lbs. weight, is in progress. This new gun we learn is fitted with a breech-loading apparatus, but no details have yet been published.

V. THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM.

AS the applications of science to industry are every day becoming more important, it may be interesting to review the origin and progress of our National Geological Survey. This institution was established for the purpose of arranging, in a form easily accessible to the public, a complete body of information respecting the geological structure of the British Islands, and the disposition and extent of their mineral wealth.

It was about forty years ago when Sir Henry (then Mr.) De la Beche proposed to the Government to publish copies of the ordnance maps geologically coloured. This proposal being acceded to, the Survey was commenced single-handed by him, in the year 1834. Having for some time previously worked at the geology of the west of England, he was the better prepared to issue geological maps of Cornwall, to which his attention was first given. Subsequently, a small branch of the Trigonometrical Survey (then under the superintendence of Colonel Colby, R.E., F.R.S.), was formed under the directorship of De la Beche.

About the same time, a geological branch of the Ordnance Survey was formed in Ireland, and placed under the charge of Captain Portlock.

In 1835, De la Beche suggested to the Chancellor of the Exchequer that a collection should be formed, and placed under the charge of the Office of Works, containing specimens of the various mineral substances used for roads, in constructing public works or buildings, employed for useful purposes, or from which useful metals were extracted. In 1837, the sanction of the Treasury was given to this design, and a building in Craig's Court was devoted to the work of the Office and the reception of the specimens. This was replaced by the more suitable building now occupied at Jermyn Street, the Museum of Practical Geology, which was opened to the public in 1851.

In Ireland, the specimens were first formed into a museum at the Ordnance Survey Office in Belfast, and afterwards transferred to Dublin, where they are now placed in the Museum of Irish Industry.

In 1845, the Geological Survey was transferred from the Ordnance Survey to the charge of the Chief Commissioner of Her Majesty's Woods, Works, and Land Revenues; and an Act of Parliament was passed, giving the necessary powers to all duly appointed officers of the Survey to examine every portion of the country without fear of being prosecuted as trespassers on private property.

Professor A. C. Ramsay was appointed the first Local Director for Great Britain, and Captain (now Sir Henry) James for Ireland, acting under De la Beche as Director-General. Meanwhile the Office of Woods and Forests was modified, and, on the formation of the Department of Science and Art in the year 1854, the Geological Survey of the United Kingdom was consigned to it, at first under the Board of Trade, and afterwards under the Committee of Privy Council for Education.

Since this period, great changes have taken place in the direction and organisation of the Survey, while the staff has been largely increased. The Director-General, Sir Henry De la Beche, died in 1855, and was succeeded by Sir Roderick Murchison, who died in 1871. Professor Ramsay then received the appointment, and Mr. Bristow was made Director for England and Wales. Captain James (now Director of the Ordnance Survey) was succeeded in Ireland by Dr. Oldham (now Superintendent of the Geological Survey of India), Dr. Oldham by Mr. Jukes, who died in 1869, and he by Mr. Hull. Scotland, too, has been severed from England, and Mr. Geikie appointed Director.

As at present constituted, therefore, the Geological Survey of the United Kingdom is under the Director-Generalship of Professor A. C. Ramsay, LL.D., F.R.S. Mr. H. W. Bristow, F.R.S., &c., is Director for England and Wales; Mr. Edward Hull, M.A., F.R.S., is Director for Ireland; and Professor Archibald Geikie, LL.D., F.R.S., for Scotland. The field-staff embraces two district-surveyors, eight geologists, twenty-four assistant-geologists, and one fossil collector, in England; one district-surveyor, three geologists, nine assistant-geologists, and two fossil-collectors, in Ireland; and one district-surveyor, two geologists, six assistant-geologists, and two fossil collectors in Scotland. In addition to these officers, Mr. R. Etheridge, F.R.S. (Palæontologist), and Professor T. H. Huxley, LL.D., F.R.S. (Naturalist),

with two assistants, have the naming and arranging in the museum at Jermyn Street of the fossils collected on the Geological Survey of England and Wales.

The results of the Survey operations will be learnt from the published maps, memoirs, and sections. The following statistics show the present state of the progress of the Geological Survey.

The whole of Wales has been completed on the one-inch scale, while in England twenty-five counties have been finished. The area which remains to be surveyed comprises portions of Northumberland, Cumberland, Westmoreland, Durham, Yorkshire, Lancashire, the Isle of Man, Lincolnshire, Nottinghamshire, Leicestershire, Cambridgeshire, Huntingdonshire, Norfolk, Suffolk, and Essex. In England and Wales, which comprise 110 sheets, 80 complete sheets have been published on the one-inch scale; while numerous maps on the scale of six inches to one mile have been published to illustrate the coal-fields of Yorkshire, Northumberland, Durham, and Lancashire. A number of sheets adjacent to the Yorkshire coal-field, and not intended for publication, are deposited for reference at the Geological Survey Office, where they can be seen, and (under certain conditions) copies may be obtained. Portions of the western counties, Gloucestershire and Somersetshire, have been re-surveyed in greater detail.

In Ireland, which comprises 205 sheets on the one-inch scale, 135 sheets have been published, and what remain to be finished comprise portions of Galway, Mayo, Roscommon, Sligo, Leitrim, Fermanagh, Cavan, Monaghan, Tyrone, Donegal, Londonderry, Antrim, Down, Armagh, and Louth. All these maps were surveyed on the scale of 6 inches to a mile, and reduced for publication. Altogether seventeen counties have been completed.

In Scotland, which comprises 120 sheets on the one-inch scale, 18 maps have been published, illustrating the geology of portions of Wigtonshire, Ayrshire, Kirkcudbright, Dumfriesshire, Lanarkshire, Renfrewshire, Peeblesshire, Dumbartonshire, Stirlingshire, Linlithgowshire, Edinburghshire, Haddingtonshire, Berwickshire, Fife, and Kinross. Maps on the 6-inch scale have been published to illustrate the coal-fields of these counties.

Numerous horizontal sections drawn to the scale of 6 inches to the mile, and vertical sections, on a scale generally of 40 feet to an inch, have been published to illustrate the geological structure.

Memoirs and Explanations, containing accounts of the

stratigraphical relations of the rocks, their characteristic fossils, and notes on the mines and minerals accompany most of the maps. Special memoirs on large areas, and detailed descriptions of fossils have also been published.

From three to four thousand square miles are annually surveyed in the United Kingdom. There is, however, much old ground to be gone over in mapping the superficial deposits, which not only have an important economic value in many instances, but are also intimately connected with questions of health, of drainage, and water-supply.

The Museum at Jermyn Street well illustrates the applications of geology, by exhibiting a series of rocks and minerals, and their adaptation to purposes of use and ornament. An extensive palæontological collection likewise illustrates the geological maps; the study of fossils proving an important guide in the identification of strata.

It is by studying the Maps, Sections, and Memoirs together, that the great practical value of the Survey is understood. The Maps themselves will show the superficial extent of the different strata, whether gravel, sand, clay, limestone, slate, sandstone, marl, or alternations of these rocks, such as clay and limestone, sand and gravel. The colours representing these geological formations are an indication of position and age. To learn their thicknesses, mineral characters, &c., the Memoirs must be consulted; while to understand their underground extension, the Sections will prove necessary.

Hence the applications to Agriculture, Engineering, and Architecture, and still more to Mining, will be at once apparent.

It is needless to remark upon the fruitless trials for coal which have been made even in recent years. The late Professor Jukes has stated that the money wasted in such searches, of which he had been personally cognisant, could not have been less than £150,000. The Geological Survey has checked much of this fruitless expenditure.

Some of the more important results of the Survey are shown in the Report of the Royal Coal Commission. The area of the exposed coal-measures of England is estimated at about 2840 square miles. The investigations of Professor Ramsay have led him to conclude that 3141 square miles of coal measures are present beneath the Permian and Triassic strata—301 square miles more than the area of our exposed coal-fields!

VI. GALL'S DISCOVERY OF THE PHYSIOLOGY OF THE BRAIN, AND ITS RECEPTION.

By T. Symes Prideaux.

"Strictly speaking you only play the part of puppets in a show; when certain cerebral organs are put in action, you are led according to their seat to take certain positions, as though you were drawn by a wire, so that we can discover the seat of the acting organs by the motions. I know that you are blind enough to laugh at this; but if you will take the trouble to observe, you will be convinced that by my discovery I have revealed to you more things than you were aware of."—"Gall, *in a familiar Letter to his Friend* Baron Retzer, 1798."

If we are to accept the verdict passed amidst mutual congratulations by the Physiologists of the period assembled at Bradford, we are on the eve of obtaining a revelation of the physiology of the brain by the localised application of electricity to its surface. Facts carefully observed and accurately recorded must always possess an intrinsic value, but it is possible to err in their interpretation; that this has been done to some extent with reference to the experiments in question, and exaggerated expectations founded on misconception indulged in as to the amount and accuracy of the knowledge to be expected from this source, is to me abundantly clear.

Enthusiasm in the pursuit of knowledge is doubtless amongst the highest of the characteristics which distinguish the noblest specimens of humanity from the common herd of mankind. As an evidence of mental activity, the jubilation with which the announcement of the results of applying electricity to the surface of the brain has been received is in the highest degree satisfactory. The more cordial the reception accorded these experiments, however, the more prominently the question obtrudes itself,—What are the distinctive differences in the path pursued to attain one common object by Fritsch, Hitzig, and Ferrier, and the method of Gall, that should occasion the results of the former to be welcomed with acclamation, whilst those of the latter were received with the hail of sneers, scoffs, ridicule, misrepresentation, and contumely? To the student of the human mind the difference, or rather contrast, offers a curious and interesting problem.

Can we find a partial explanation of the anomaly in the more purely physical character of the recent method of research—that the subject of attention in the one case is a movement visible to the senses, in the other a mental quality, an abstraction which presents no sensuous object to the mind? What is certain is, that many men have great

taste and capacity for the observation, description, and arrangement of material facts, who are singularly deficient in the power of contemplating abstract existences. The majority of men appear to require a physical substratum for their thoughts. Their ideas are almost limited to *images*, or pictures of outward objects presented by the external senses; or secondly, to *conceptions* of actions being a change in the relation of material objects; or thirdly, to bodily sensations arising from the action of the external senses. Either the specialised senses—taste, smell, hearing, and sight—or the diffused sense of feeling, co-extensive with the surface of the body, and hence adopted as a generic term, and applied metaphorically (with its opposite poles, pleasure and pain) to all internal affections. They have not adequate power of abstraction to separate the subjective from the objective. Not analytical power sufficient to dig phantoms from their consciousness, isolate them from their surroundings, and hold them continuously before their mental vision for contemplation. They catch a glimpse of a figure for a moment, but before they have time to study its features it dissolves away like a wreath of mist. Now the subject matter of Phrenology is mental qualities, not material objects; whilst, in addition to its abstract basis, it superadds the doctrine of the dependence of the mental functions on certain external relationships of form and size, successfully to appreciate which demands an amount of preliminary study hardly likely to be expended on the problem, by those to whom one of the factors in the equation presents the aspect not merely of an unknown, but of an incommeasurable quantity. *Non omnia possumus omnes*, indeed, it is usually those whom some predominating instinct prevents being too discursive and keeps in one path of study by whom additions to the sum of human knowledge are made. Let us be thankful to the student, whose range of thought is limited to objects of sense, for his contributions to his own department; but do not let us regard him as an authority in others, nor commit the shallow blunder of citing his indifference to, or disbelief in, the invisible rays at the higher extremity of the spectrum as an argument for their non-existence. We have cultivators of the physical sciences, mathematicians, astronomers, natural philosophers, chemists in abundance, plenty of naturalists, ready to seize and describe all the peculiarities in form, size, weight, colour, distribution, and habits, of everything that has life. We have even a limited supply of metaphysicians and psychologists, who deal with *abstractions* and

words in contradistinction to *things*, and inhabit an ideal world of their own. The dealers in things and the dealers in abstractions mostly dwell apart, and too often regard each others' pursuits with ill-disguised contempt.

Now the phrenologist requires to unite to a considerable extent the capacities and tastes of both classes; to combine the powers of mental analysis—the facility for detaining abstractions before the mind's eye for study—of the metaphysician and psychologist, with the instinct of observation and quick perception of physical differences by which the naturalist is distinguished—and in the fact that individuals who combine the two phases of capacity will be less numerous than those who possess one of the qualifications singly, we see an explanation of the cause why the scientific cultivators of phrenology are fewer in number than either the physicists or the metaphysicians.

In scanning the causes of the hostility Phrenology has so widely encountered, amongst others we must not omit to notice its close bearing on the personality of individuals. Men with little heads, little minds, but great vanity, rebel against a standard of capacity which gauges them correctly. A science which renders it possible—

> . . . . . " A des signes certaines
> Reconnaitre le cœur des perfides humaines,"

will always have antagonists to whom such an idea is distasteful. The whole of the genus humbug, the empirics and impostors of the day, and men conscious of being at bottom thoroughly dishonest and unprincipled, instinctively recoil from a system which threatens to unmask their moral deformities to the eyes of the world, and reveal their true features, despite a whole wardrobe of trappings of duplicity. Napoleon boasted of having greatly contributed to put down Gall. His own medical attendent, Corvisart, one of the greatest physicians France ever produced, was an admirer of Gall, and vainly endeavoured to introduce him to the Emperor. "Corvisart," says Napoleon, "was a great partisan of Gall, and left no stone unturned (fit l'impossible) to push him on to me, but there was no sympathy between us." In short, Napoleon confessed he felt the greatest aversion for those "who taught that Nature revealed herself by external forms."

Again, the bulk of mankind have no doubt been organised by nature to lead a life of action, to do, and not to think. In youth they are plastic, and readily receive the impression stamped by their teachers, but by mature age the receptivity of childhood has vanished, and the clay of which they are

composed refuses all attempts to mould it afresh; and especially is this the case where the egotistic feelings of self-love and vanity outweigh the pure love of knowledge for its own sake. Such men may indeed imbibe new ideas, and acquire an increase of knowledge as they grow older, but the new knowledge must have some points of affinity and harmony with the old, to be cordially welcomed. Above all, it must not threaten the subversion of those existing canons of belief which have hitherto guided them on life's journey, or it will infallibly excite antipathy and antagonism. Every day we have the spectacle of the direct testimony of facts being ignored and rejected without examination, from the inference that they are opposed to some cherished belief. Even the scientific *par excellence*, the professed philosophers, are not exempt from this human frailty; touch but the ark that enshrines the object of their worship, and you shall see the bigotry and intolerance with which they credit the theologian rivalled, if not outdone. As at the advent of Phrenology it encountered the antagonism of the religious world from its supposed tendency to materialism; so, at the present hour, many of our leading physicists shut their eyes to the curious phenomena of (the so-called) spiritualism, and open their mouths to assail its investigators, because they fear that these phenomena clash with that materialistic philosophy which constitutes the staple article of their scientific creed.

How vast a portion of our present stock of scientific knowledge would be non-existent if no one had been found to "take an interest" in the phenomena of magnetism! and can the most bigoted apostle of the new positive-physical gospel venture to assert that a domain of fact as wide in its extent and fruitful in its result may not lie hidden, awaiting conquest by man in this force of source unknown, the conditions attending the presence of which, though yet undiscovered, we may be assured are governed by laws as definite and immutable as those of gravitation. We do not yet know how to multiply mediums at pleasure, as we do magnets, because we know neither the species of loadstone nor the kind of manipulation required, but all honour to those who are engaged in the research.

Apparently as long as psychologists were content to frame theories out of their own consciousness, and confined themselves to abstractions, their researches created no antagonism in the physicists who occupied themselves with the study of material objects and their properties and functions; but when these saw their own peculiar province invaded, and the physiology of the highest organ of the

body, to them an enigma, for the solution of which neither their tastes nor capacities were adapted, declared to be unravelled by a method of study for which they had no proclivity, and by an individual who had altogether surpassed them in their own province of anatomy, their pride rebelled, and their wounded *amour propre* found vent in denunciations as outrageous and absurd as ever greeted the author of a new discovery. English metaphysicians, and immaterialist divines also, led by English anatomical authorities to regard the propounder of these new doctrines as an ignorant quack, were not slow in joining the chorus of detraction and abuse against the audacious innovator, who overthrew all their cherished theories as to the independence of the mind on organisation—the former viewing the doctrines of Gall with profound contempt and disgust as tending to degrade man to the level of brutes, the latter with repugnance and alarm as threatening to sap the foundations of religion.

Dr. John Gordon, a lecturer on anatomy of great reputation in Edinburgh, in an article in the "Edinburgh Review," in 1815, said, "We look upon the whole doctrines taught by these two modern peripatetics (Drs. Gall and Spurzheim), anatomical, physiological, and physiognomical, as a piece of thorough quackery from beginning to end." Lord Jeffrey, in the same periodical, in 1826, designated the doctrines as "crude," "shallow," "puerile," "fantastic," "dull," "dogmatic," "incredibly absurd," "foolish," "extravagant," and "trash." The "Quarterly Review," in their notice of Madame de Stael's "L'Allemagne," censured her for being "by far too indulgent to such ignorant and interested quacks as the craniologist Dr. Gall," and in No. XXV. the same Review declared the new science to be "sheer nonsense," and designated Dr. Spurzheim as "a fool." The Rev. Thomas Rennell, Christian advocate at Cambridge, in his "Remarks on Scepticism, especially as it is connected with the subjects Organisation and Life," assures his readers that the system of Gall and Spurzheim "is annihilated by the commonest reference to fact," spoke of "its absurdities," of this "masterpiece of empiricism," and designated it as "the flimsy theories of these German illuminati." Whilst as late as 1836, Sir CharlesBell wrote—"The most extravagant departure from all the legitimate modes of reasoning, although still under the colour of anatomical investigation, is the system of Dr. Gall. Without comprehending the grand divisions of the nervous system, without a notion of the distinct properties of the individual nerves, or having made any distinction of the columns of the spinal marrow,

without having even ascertained the difference of cerebrum and cerebellum, Gall proceeded to describe the brain as composed of many particular and independent organs, and to assign to each the residence of some special faculty."

The insular ignorance of Gall's anatomical discoveries, position in the scientific world, and true character displayed in these insulting criticisms, is no less disgraceful than astounding. Professor Hufeland, an anatomist and physiologist of European reputation, thus expresses himself concerning Gall:—"It is with great pleasure and much interest that I have heard this estimable man himself expound his new doctrine. I am fully convinced that he ought to be regarded as one of the most remarkable phenomena of the 18th century, and that his doctrine should be considered as forming one of the boldest and most important steps in the study of the kingdom of nature. One must see and hear him to learn to appreciate a man completely exempt from prejudices, from charlatanism, from deception, and from metaphysical reveries. Gifted with a rare spirit of observation, with great penetration and a sound judgment, identified, as it were, with nature, he has collected a multitude of signs of phenomena which nobody had remarked till now—has discovered the relations which establish analogy between them—has learnt their signification—has drawn consequences and established truths, which are so much the more valuable that, being based on experience, they emanate from nature herself."

"The worthy Reil," says Professor Bischoff, "who as a profound anatomist and judicious physiologist stands in no need of my commendation, has declared, in rising above all the littleness of egotism, that he had found more in the dissections of the brain performed by Gall than he had conceived it possible for a man to discover in his whole life-time!"

"Loder," continues Professor Bischoff, "who certainly does not yield the palm to any living anatomist, has expressed the following opinion of the discoveries of Gall:—'The discoveries of Gall in the anatomy of the brain are of the highest importance, and many of them possess such a degree of evidence that I cannot conceive how any one with good eyes can mistake them. I refer to the great ganglion of the brain—to the passage of the corpora pyramidalia into the *crura* of the brain and the hemispheres—to the *fasciculi* of the spinal marrow—to the crossing of the fibres under the pyramidal and olivary eminences—to the recurrent fibres of the cerebellum—to the commissures of the nerves—

to the origin of the motor nerves of the eyes, of the trijeminal nerves, of those of the sixth pair, &c. These discoveries alone would be sufficient to render the name of Gall immortal; they are the most important which have been made in anatomy since the discovery of the system of the absorbent vessels. The unfolding of the brain is an excellent thing. What have we not to expect from it as well as from the ulterior discoveries to which it opens the way? I am ashamed and angry with myself for having, like the rest, during thirty years, sliced down hundreds of brains as we cut a cheese, and *for having missed seeing the forest on account of the great number of trees it contained.* But it serves no purpose to distress one's self, and to be ashamed. The better way is to lend an ear to truth, and to learn what we do not know.'"

Which latter piece of advice I commend to the notice of those little great men, the eminent compilers, who,—devoid of the original genius which, by perceiving relationships before unknown and unsuspected, confers new principles on science,—would fain set themselves up as physiological authorities on the strength of their book-making capacities.

Not only were the great and important additions made by Gall and Spurzheim to the anatomy of the nervous system fully admitted by Cuvier, but their position as the highest authorities on the subject was so fully recognised in Paris, in 1813, that the article, "Anatomie du Cerveau," for the "Dictionnaire des Sciences Medicales," was confided to their care.* All English anatomists, however, have not followed the suit of Dr. John Gordon and Sir Charles Bell, in recording at once their jealousy and their ignorance by absurd denunciation of Gall. Mr. Grainger, the greatest English authority of his day on the anatomy of the brain and spinal cord, writes, "The true anatomy of the cerebrum was perfectly unknown till the researches of Gall, and it is due to the character of this eminent man and of his pupil, Spurzheim, to state that all our knowledge of the anatomy of both the brain and spinal cord has resulted from their inspections;" and Joshua Brookes, in his lectures, and Mr. Solly, in his well-known work on the anatomy of the brain, have done full justice to the anatomical discoveries of Gall.

* The necessary result of the old method of dissecting the brain is thus pithily described in this article:—"On a mis en usage une méthode de dissection très-défectueuse; on ne fesait que des coupes horizontales, verticales, ou oblique, par en haut ou par en bas et on enlevait successivement des tranches de cet organe. De cet manière, *on commencait par détruire les connexions des différens appareils et on procédait sans égard pour l'ordre dans lequel les parties se suivent naturallement.*"

The method pursued by Gall, in seeking to ascertain the functions of the brain, was by comparing the power of manifesting particular mental faculties with the size and condition of particular portions of this organ. Phrenologists believe this method to be vastly superior to all others, and, in justification of this opinion, point to the rich harvest it has produced in contrast to the barren results which have hitherto been obtained by the employment of mutilations and the application of stimuli. Is there, at the present moment, a single physiologist in a position to declare that, after qualifying himself to judge of the development of the organs by the requisite study, the result of careful examination has convinced him that the localities assigned by Gall to the primitive mental faculties are erroneous? Why is this sound and legitimate mode of studying the functions of the brain neglected and ignored by physiologists in general, "who seem desirous of exhausting every possible variety of error before they will adopt it?" Men of science are usually eager to avail themselves of every practicable means in the pursuit of knowledge, but it would appear to be a desideratum to discover the functions of the brain by other than phrenological methods.

In addition to employing mutilations, Rolando trephined the cranium of various quadrupeds, and applied one of the poles of a voltaic pile to different portions of the brain, whilst the other was applied to different parts of the body. With reference to these experiments of Rolando, and the experiments by mutilation of Flourens, Gall remarked:—

"It is a subject of constant observation that, in order to discover the functions of the different parts of the body, anatomists and physiologists have always been rather disposed to employ manual means than to accumulate a great number of physiological and pathological facts,—to combine these facts, to reiterate them, or to await their repetition in case of need,—and to draw slowly and successively the proper consequence from them, and not to announce their discoveries but with a wise reserve. This method, at present the favourite one with our investigating physiologists, is imposing from its materiality; and it gains the approbation of most men by its promptitude and its apparent results. But it has also been constantly observed that what has appeared to have been incontestably proved by the mutilator A., either did not succeed with the mutilator B., or that he had partly found in the same experiments all the proofs necessary to refute the conclusions of his predecessor. It is but too notorious that similar violent experi-

ments have become the scandal of the Academicians, who, seduced by the attraction of ingenious operations, have applauded with as much enthusiasm as fickleness the pretended glorious discoveries of their candidates.

"In order that experiments of this kind should be able to throw light on the functions of each of the cerebral parts, it would require a concurrence of many conditions impossible to be fulfilled. It would first require that we should be enabled to restrain all the effects of the lesion to that portion only on which the experiment is performed; for if excitement, hæmorrhage, inflammation, &c., affect other parts, what can we conclude? and how can we prevent these inconveniences in mutilations either artificial or accidental? It would be necessary that we should be able to make an animal whose brain has been wounded and mutilated—who is filled with fear and suffering, disposed to manifest the instincts, propensities, and faculties, the organs of which could not have been injured or destroyed. But captivity alone is sufficient to stifle the instincts of most animals."

Have the results attained by the recent experiments of Fritsch, Hitzig, and Ferrier a tendency to invalidate these opinions of Gall, or do they not rather confirm their correctness? I presume it will hardly be pretended that the function of a single portion of the brain has yet been discovered by these means,* and I venture to think there is but little probability of their effecting such a discovery in the future, notwithstanding the exaggerated expectations held out. At present it is palpable that physiologists are quite adrift as to the real signification of the phenomena elicited, the true interpretation of which must be sought in the discoveries of Gall, who maintained the competency of the surface of the brain to originate muscular movements in opposition to the current doctrines of physiology and the asserted proof to the contrary afforded by the experiments of Flourens, and other mutilators, and whose familiarity with the fact is recorded in the extract from his letter to Baron Retzer, in 1798, prefixed to this article.

The explanation of the phenomena obtained by the application of stimuli to the surface of the brain, is found in the fact that those innate faculties which require the aid of the

* A fact conclusive on this point, and which places in a striking light the vagueness and want of precision of the results obtained, is the circumstance that that eminent compiler, Dr. Carpenter, sees in these experiments "a remarkable confirmation" of his transcendently absurd and ridiculous notion that the intellectual organs are seated in the back of the head.

muscular system to carry out their behests have the power of originating the movements necessary for this purpose; and hence, when Dr. Ferrier applied a galvanic current to the cortical surfaces of the organs of the instinct "to take food," "to seize prey," "to destroy," "to fight," "to construct,"—movements "of mastication," "of striking with the claws, or seizing with the mouth," "of biting and worrying," "of scraping, or digging," ensued: whilst the stimulation of the same locality (constructiveness) which put the fore paws and hind legs in action in the rabbit, would, in the beaver, superadd the motion of the incisor teeth and the tail. What can be more palpable than that the inferences to be obtained from such experiments are not only far more vague and indefinite than those furnished by the employment of the phrenological method, but absolutely incapable of ascertaining the shape, and defining the boundaries, of the organs, as has been accomplished by Gall in the case of locality, the shape of which he ascertained to be similar in dogs to its form in man. In short, little more can be said on behalf of these experiments at present than that in a cloudy and obscure form they lend a vague general confirmation (not required) to the correctness of the localities assigned to the primitive faculties by phrenologists.

Amongst the many eminent men whose researches and discoveries have shed honour on the profession of medicine, Gall will assuredly by posterity be accorded a place second to none. Man had looked on man, and scanned the face of his brother in sunshine and in storm, in friendship and in anger, for countless thousands of years, without having succeeded in seizing and individualising a single primitive faculty, much less in discovering its seat. The advent of Gall broke up the long night of darkness and error as to their own being, under which the human race had slumbered for ages. Sensation, perception, memory, judgment, imagination—the idolæ of the past—the stock properties of every psychological system from that of Aristotle downwards, instead of being primitive faculties, were clearly demonstrated by the most masterly analysis and the most unanswerable arguments to be simply different degrees or consecutive modes of action proper to each of the elementary intellectual faculties, and necessarily variable in strength in relation to subjects specifically distinct. Gall studied the maximum or minimum exhibition of certain passions or capacities compared with the extreme or defective development of certain parts of the brain; and when a vast number

of concurrent experiences had satisfied him of a connection, named the primitive faculty by the simplest words indicative of its function to be found in the vocabulary of every-day life. He thus replaced the phantoms of the metaphysicians, which explained nothing, by terms which speedily asserted their vitality by being constantly heard in the mouths of the people to assist them in defining and describing their fellow-men, thus at once obtaining that sanction from the spontaneous dictates of popular common-sense, which is the surest test of the truth of all fundamental ideas.

It is a common doctrine that discoveries are seldom made by an individual greatly in advance of the scientific mind of the day, or without other investigators having been placed by the existing state of knowledge on the same track as the more fortunate discoverer, who is thus merely credited with having by a short date anticipated other investigators in bestowing a new fact, or idea, on mankind. With regard to the discovery of phrenology, however, made at the close of the last century by Dr. Gall, if we may judge by the fact that what he discovered the great mass of his contemporaries never succeeded in recognising, even when the locality for research was pointed out to them, and the means of observation lay in profusion everywhere around, there appears every reason to believe, that but for the appearance of a man of his rare and exceptional genius, the vast contribution to human knowledge for which the world is indebted to his labours would still have been slumbering in the womb of futurity. I venture to assert that no body of doctrines were ever established on a series of observations more cautiously conducted, rigorously scrutinised, and patiently verified than those of phrenology by Dr. Gall, who devoted his entire life and all his pecuniary resources to this object, finding his reward in the consciousness of the importance of his discoveries, and that prophetic vision of the future which placed him above contemporary jealousies, and caused him to exclaim in calm self-reliance, "This is *truth*, though opposed to the philosophy of ages!"

"I have always," says Gall, "had a consciousness of the dignity of my researches, and of the extended influence which my doctrine will hereafter exercise on all the branches of human knowledge, and for this reason I remain indifferent to all that may be said either for or against my works. They differed too much from the received ideas of the times to be appreciated and approved at first. . . . . My views of the qualities and faculties of man are not the fruit of subtle reasonings. They bear not the impress of the age

in which they originate, and will not wear out with it. They are the result of numberless observations, and will be immutable and eternal like the facts that have been observed and the fundamental powers which these facts force us to admit."

VII. ECONOMY OF FUEL.

By Frederick Charles Danvers, Assoc. Inst. C.E.

THE important question of fuel economy is one which has, within the last few years, attracted the attention of several of our learned societies, and it is one which cannot fail to grow in importance rather than diminish. Practically speaking, coal is the only fuel upon which we can with any certainty rely for our great manufacturing industries; and although this may, in some measure, be supplemented by a more extensive use of peat than has hitherto been the case, still coal must continue to be looked upon as the great staple upon which the chief of our manufactures must continue to depend. Coal, as we have shown upon several former occasions, exists only to a limited extent, and our existing resources of that fuel are not capable of being reproduced, as is the case with wood, and to some extent also with peat; and, in order to make our coal supply keep pace with the ever increasing demand upon it, one of the most important considerations now is, how so to economise its use in all branches of manufacture and for steam and domestic purposes as to minimise the amount of waste which at the present time takes place in the use of it. With the view, therefore, of pointing out what may be done in that direction, we purpose now to consider how far scientific and mechanical improvements have already succeeded in that direction, and to what extent further economies may be possible.

Each pound of coal possesses a certain number of heat units, and to produce any given results from its combustion requires the development of a given number of heat units; when, therefore, we find that in order to produce such results a considerable number of heat units are expended in excess of the theoretical requirements, it is very evident that a certain amount of avoidable waste is taking place. It is not to be expected that the full economic value of coal can ever be attained in most cases, for were such the case it would necessarily follow that the escaping volumes from

combustion must only be permitted to escape when all the heat given out in combustion has been effectually abstracted, and all radiated heat would also require to be reserved for useful purposes.

There can be no doubt that coal burnt under a certain amount of compression will yield the best results; but the important question is how to regulate the admission of air so that no more is employed in supporting combustion than is actually required for that purpose; any excess above that amount must tend only to lower the temperature, and so abstract a certain amount of useful effect from the fuel. The air admitted for this purpose must also be so regulated as to ensure its complete combination with the fuel, so that no air in a free state shall pass away undecomposed. The channel for escaping vapours should also be so regulated in size as to give no more than sufficient space for their free passage in a certain state of expansion, whilst the current of air into the furnace must also be maintained at a rate sufficient for the combustion of the proper amount of fuel for the work to be effected. Now, in order to maintain this state of things, it is necessary to produce an upward current in the chimney, and in order to do this, the ascending vapours must either be forced out by mechanical means, or they must be allowed to escape at a temperature above that of the atmosphere. In either case, therefore, it is clear that a certain amount of heat must be developed in excess of what would otherwise be required for the purpose. To obtain, therefore, the full theoretic value of the coal burnt for any specific purpose must be, under these considerations, absolutely impracticable; the nearer we attain to that point, however, the less cause of complaint there will be of a waste of coal taking place in any particular case. As we shall presently see, the use of hot-blast in iron smelting was followed by a decided saving in the amount of fuel required to produce 1 ton of pig-iron, notwithstanding that a certain portion of it was consumed in first heating the air for the blast. It will readily be understood how the introduction of cold air into any furnace must have the immediate effect of lowering the temperature, and it is found that the amount of fuel necessary to heat the air before admitting it into the furnace is less than the amount required to maintain the temperature within the furnace when it is fed with cold air. A free current for escape of the vapours of combustion is followed by the escape of the more volatile portions of the fuel used, which are therefore absolutely lost; whilst, with a strong blast, where there is a free escape—as in a locomotive—small particles of fuel, wholly uncarbonised, are also carried

away without doing any effective work. Although, in both these cases, the greater portion of the fuel remains behind until it is wholly consumed, only a small proportion of its effective value is retained, whilst the greater part is lost, representing so much waste of fuel, a great portion of which must be looked upon as avoidable loss.

To turn now from generalities to the practical working of the question, we shall proceed to consider first the application of these principles in the manufacture of iron, which branch of industry alone consumes about one-third of the total amount of coal raised in the United Kingdom, and which, therefore, most largely affects the great coal question of the present day.

Taking the results of iron manufacture in Scotland, we find, upon the authority of Dr. Percy, that the ton of pig-iron, as made in 1829 at the Clyde Iron Works, required the coke of 8 tons $1\frac{1}{4}$ cwts. of coal; whilst in the following year, owing to Neilson's invention of the hot-blast to iron furnaces, the introduction of air heated to 300° F. brought down the consumption per ton of pig to 5 tons $3\frac{1}{4}$ cwts. 8 cwts. of coal were consumed in heating the blast, so that the actual saving per ton of pig-iron was $2\frac{1}{2}$ tons. In 1833, when raw coal had come to be used instead of coke, 1 ton of pig-iron was made with 2 tons $5\frac{1}{2}$ cwts. of coal, which, with 8 cwts. for heating the blast, made a total of 2 tons $13\frac{1}{2}$ cwts. Hence by the application of the hot-blast, the same amount of fuel reduced three times as much iron, and the same amount of blast did twice as much work as previously.

Subsequently to the attainment of the foregoing results an increase in the size of the blast-furnace has been followed by still further economy in fuel used in the manufacture of iron. The discovery of this fact is due to the iron smelters of Cleveland. When the first blast-furnace was erected in that district by Mr. John Vaughan, in 1851, the practice of older districts was followed, and the furnace was made 42 feet high by 15 feet diameter at the bosh. Up to 1858 there was a gradual increase of size, the furnaces that year being 56 feet in height by 16 feet bosh. The results of this increase of size were so satisfactory that Mr. Vaughan was led to rebuild one of the old furnaces, increasing its size to 61 feet high by 16 feet 4 inches bosh. This may be said to be the first decided step towards the great increase in size which followed, the comparative results being so much in favour of the large furnace over the original small one that it soon became an undoubted fact that economy was to be found in that direction. Although the scientific reasons

which led to a saving of fuel through an increase in size were at that time not clearly understood, yet the practical results obtained were so beneficial that they culminated in a revolution unparalleled in the blast-furnace history of any district, in which all the original furnaces and plant were razed to the ground, and new ones on the now established improved principles were built in their stead. Furnaces have been built over 100 feet in height, and some of them as wide as 30 feet in the bosh; and it is the opinion of those best competent to judge of the matter, that the useful maximum of both height and diameter have been attained, if not exceeded. The object of increasing the size of the Cleveland furnaces was twofold; first, to increase the make; and, secondly, to economise fuel; a third has followed gratuitously, viz., improvement in quality. The saving of coke from this cause has been considerable, and may be put down at from 7 to 8 cwts. of coke per ton of iron made. Mr. Isaac L. Bell, in investigating the causes which led to this economy of fuel in the larger furnaces, discovered that in a furnace one or two combinations are possible—that of one equivalent of carbon uniting with one or with two equivalents of oxygen; and that in the latter case as much heat is developed by 20 cwts. as is done by 71·14 cwts. when the carbon only unites with one equivalent of oxygen.

The cause of the saving in fuel in large furnaces is two-fold; first, by the interception of a considerable portion of the heat formerly carried away in the gases—the products of combustion of the old furnaces; and, secondly owing to a better state of oxidation or combustion of the carbon,—a state of things proved by a great number of chemical analyses of the gases themselves as they leave the furnaces. Mr. Bell has also proved that no subsequent additions to the size of the furnace, beyond a certain point, has been attended with anything like the saving which accompanied the first steps in that direction; and that this is due to the fact that the escaping gases have, by such increase in dimensions, been deprived of nearly all the heat they can be made to surrender for use in the furnace; and that the chemical action in a furnace of about 12,000 feet is as perfect, so far as numerous analyses of the gases could indicate, as it is in a furnace of 25,000 cubic feet.

Next to the increased size of the furnace, the principal cause of economy in fuel in the manufacture of iron is the improved temperature of blast at the tuyeres, which has been increased up to 1400°. In order to obtain this

temperature, heating-stoves are employed, consisting most generally of a series of iron pipes within a furnace, through which the heat for the blast is drawn. The highest temperature and the best results have, however, been obtained with Whitwell's hot-blast fire-brick stoves, by the use of which, at Consett, iron has been made with 17 cwts. 2 qrs. of coke per ton, the blast being at a pressure of 3 lbs. per inch, and the temperature about 1400°. With an increased pressure of blast to 4 lbs., and a decrease in temperature to 1200°, an increased production of pig-iron was the result, but the consumption of coke rose to 19 cwts. 2 qrs. per ton.

Under the most economical system of working, with open-topped furnaces, an enormous amount of fuel is wasted by the escape of the vapours of combustion, many of them only half consumed at a high temperature. So early as the beginning of the present century (1811), the important practical problem of the utilisation of the waste gas of iron-smelting furnaces was solved in a satisfactory manner in France; but upwards of five and twenty years elapsed before it began to attract the serious attention of iron-masters in Great Britain or on the Continent of Europe. The first attempts made in this country were exceedingly crude, and much of the carbonic oxide was allowed to escape unburnt into the air, by which an enormous amount of heat, capable of being developed by the combustion of that gas, was lost. The calorific effect of the waste gas is due partly to its sensible heat, and partly to the heat developed by its combustion in contact with atmospheric air. In some furnaces, the gas is taken off through several circular openings at a short distance below the level of the solid contents of the furnace, their exhaustion being effected by means of a high stack. In others, there is an annular passage or flue near the mouth, extending all round, and communicating with the interior by several short passages, and in this case, also, the aid of a stack is required for exhaustion. The most general method is, however, to close the top of the furnace with a "cup and cone."

We must not close our account of economy of fuel in the blast-furnace without some reference to Ferrie's self-coking blast-furnace. In considering the problem of utilising the gases escaping from blast-furnaces worked with raw coal, it occurred to Mr. Ferrie that much of the difficulty would be overcome if the coal could be coked in the furnace in somewhat the same way as it is coked in gas retorts. In the application of these ideas to a large furnace at the Monkland Works, the mouth of the furnace is closed by a bell and

hopper, and the gases are led off to the blast-heating stoves in the usual way. The upper part of the furnace, for a depth of 20 feet below the space required for the bell and cone, is divided into four compartments by vertical walls supported on arches, and radiating from the centre. These division walls, by causing additional frictional resistance to be opposed to the descent of the materials, relieve the coke formed of a portion of its load, but their main object is to enable the coking of the coal to be performed in the upper part of the furnace. The economic results obtained with this furnace have been most satisfactory, and they were thus described by Mr. Ferrie himself to the Iron and Steel Institute, at their meeting in March, 1871:—

"In the Lanarkshire district, the quantity of coal required in the manufacture of a ton of No. 1 pig-iron ranges from 50 to 52 cwts. in the furnace, whereas, in this furnace, a ton of the same quality can be produced with 32 to 36 cwts., effecting a saving in coal of nearly a ton to the ton of iron made. In ores, the saving in this furnace will be about 2½ cwts. per ton of iron."

The quantity of gas drawn off from the furnace is found to be greatly in excess of that required for heating the blast and raising steam for the blowing engines; but where works for the production of finished iron are annexed to the blast-furnaces, ready means may be found for utilising this excess of gas.

The next subject for consideration is the economy of fuel hitherto attained in the manufacture of iron and steel. Before the introduction of the puddling process, the conversion of cast-iron into malleable or wrought-iron was always effected in a finery or hearth, in which the metal was melted in contact with the solid fuel, and so exposed to the highly oxidising action of a blast of atmospheric air. Dr. Siemens, in a lecture delivered to the operative classes of Bradford, on behalf of the British Association, in September last, remarked that in the metallurgical furnace there is great room for improvement, the actual fuel consumed in heating a ton of iron up to the welding point, or in melting a ton of steel, being more in excess of the theoretical quantity required for those purposes than is the case with regard to the production of steam power or to domestic consumption. Taking the specific heat of iron at 114, and the welding heat at 2700 degrees F., it would require 307 heat units to heat 1 lb. of iron. A pound of pure carbon develops 14,500 units of heat, a pound of common coal 12,000, and therefore 1 ton of coal should bring 39 tons of iron up to the

welding point. In an ordinary re-heating furnace, a ton of coal heats only $1\frac{2}{3}$ tons of iron, and therefore produces only one twenty-third part of the maximum theoretic effect. In melting one ton of steel in pots, $2\frac{1}{2}$ tons of coke are consumed; and, taking the melting point of steel at 3600° F., the specific heat at 0·119, it takes 428 heat units to melt a pound of steel; and, taking the heat-producing power of common coke also at 12,000 units, 1 ton of coke ought to be able to melt 28 tons of steel. The Sheffield pot steel melting-furnace, therefore, only utilises one-seventieth part of the theoretical heat developed in the combustion. Several methods are now in use whereby greater economy in fuel results, but we shall not now do more than give special notice to one of these, as the object of the present article is not so much to refer to all methods of economising fuel, but rather to point out to what extent economy has been attained in various branches of consumption. We propose here merely to specify the Siemens's regenerative furnace, which is now too well known to require any detailed description, and by the use of which a ton of steel is melted with 12 cwts. of small coal, whereas in the ordinary furnace at Sheffield, about 3 tons of Durham coke are necessary to accomplish the same end. In this one operation, therefore, in the process of steel manufacture, a saving of four-fifths of the fuel ordinarily employed is capable of being effected.

Turning now to Mr. Bessemer's system of steel manufacture, we find where $3\frac{1}{2}$ tons of coke are ordinarily used per ton of steel, 3 cwts. only is required for his process; and we find that gentleman stating, in evidence, before the Coal Commission of 1871, that "if we take the present production of cast steel, in this country, by my process, at 150,000 tons a year, we should have a saving of a little over half a million tons of coke in that time, representing, of course, its proportion of coal, the amount being greater or less according to the purity of the coal employed."

In estimating the waste of fuel under steam boilers, it is necessary to remember that in burning 1 lb. of carbon in the presence of free oxygen, carbonic acid is produced, and 14,500 units of heat are liberated. Each unit of heat is convertible into 774 units of force or mechanical energy; and hence, 1 lb. of carbon represents really 11,223,000 units of potential energy; that is to say, the mechanical energy set free in the combustion of 1 lb. of pure carbon is the same that would be required to raise 11,223,000 pounds weight one foot high, or as would sustain the work which we call

a horse power during five hours thirty-three minutes. Practically, however, the results obtained fall very far short of these results. An ordinary non-expansive non-condensing engine requires commonly a consumption of from 10 lbs. to 12 lbs. of coal per horse-power per hour, whereas a good expansive and condensing engine accomplishes the same amount of work with 2 lbs. of coal per hour. In order to attain the greatest economy of fuel, used for the purpose of producing mechanical action, it is first necessary to provide such an amount of heating surface in the boiler as shall absorb all the heat produced by combustion, and transmit it to the water. The beneficial results which are attained by the greater size of boiler in relation to the coal burnt and to the horse-power required have been proved by actual usage, and are not merely matters of calculation. The Institute of Mechanical Engineers instituted a careful inquiry, in 1863, into the consumption by the best engines in the Atlantic Steam Service, and the result showed that it fell in no case below $4\frac{1}{2}$ lbs. per indicated horse-power per hour. Last year they assembled with the same object in view in Liverpool, and Mr. Bramwell produced a table, showing that the average consumption by seventeen good examples of compound expansive engines did not exceed $2\frac{1}{4}$ lbs. per indicated horse-power per hour. Mr. E. A. Cowper has proved a consumption not exceeding $1\frac{1}{2}$ lbs. per indicated horse-power per hour, in a compound marine engine constructed with an intermediate superheating vessel in accordance with his plans. Dr. Siemens has, however, proved that theoretical perfection would only be attained if an indicated horse-power were produced with about $\frac{1}{4}$ lb. of ordinary steam coal per hour.

Mr. Bramwell, in his address as President of Section G, at the meeting of the British Association at Brighton, in 1872, bore testimony to the fair duty done by locomotive engines, which he stated to be due, first, to the fact that, since the introduction of coal the furnaces have been to a considerable extent gas furnaces, with a free admission of air through open fire doors to the surface of the fuel; and, secondly, to the fact that the boilers have large absorbing powers. In marine engines there has, within the last ten years, been an enormous saving. The old fashioned engine, working at 20 lb. steam and with injector condensers, is being abandoned for engines generally on the compound cylinder principle, working at 60 lb. and 70 lb. steam, highly expansive, and fitted with surface condensers; and the result is a reduction of the consumption of fuel in the same vessels, on the same voyages, and performed in the same

time, of from 40 to 50 per cent of that which was previously burnt.

Irrespective, however, of other circumstances, a great waste of fuel may be caused by bad firing; the fire being kept too thick, or too thin, or irregular. If too thick, the carbonic acid that is generated by the combustion of the lower part of the fuel, with which the air first comes in contact, is changed in its passage through the upper part of the fuel into carbonic oxide, by absorbing from the fuel a second equivalent of carbon. If carbonic oxide gas, thus generated, does not meet with free atmospheric air, at a suitable temperature in the upper part of the furnace, it must remain unconsumed, and will pass through the flues or tubes of the boiler, and make its escape into the air, carrying with it the valuable unconsumed carbon of the coal in a gaseous form. And when it is remembered that under ordinary circumstances every pound of coal burnt into carbonic acid is capable of evaporating about 13 lbs. of water from 212°, while a pound of coal converted into carbonic oxide is capable of evaporating only 4 lbs. of water, it will be seen how necessary it is that no mismanagement of the fire should cause a portion of the fuel thus to escape unburnt up the chimney.

We have thus pointed out the extent to which, and the principal means by which economy of fuel has been attained in the manufacture of iron, and for steam purposes. The results hitherto attained are, however, still very far from what theoretically should be practicable; but there can be little doubt that the same influences which have been at work to produce established results, will continue to act in the same direction, and with equally beneficial effects, until very much better value is obtained out of coal, although it would be unreasonable to hope that the full theoretical value should ever be actually attained.; and, indeed, as has been already shown, such a result would be impossible.

Lastly, a few words with reference to the waste of fuel in domestic consumption. Nothing could be more extravagantly absurd, from an economical point of view, than the present system of open fire-places, and the method of setting them. From published returns for the year 1872 it appears that, taking the statistics of the metropolitan district as a guide, on an average, $12\frac{3}{4}$ cwts. of coal is consumed per annum for each person of the population for domestic services. In the preceding year the average was slightly higher, but prior to that it was below that average; so that it appears, in the use of fuel for domestic purposes, so far from there having been

any attempt at economy, the reverse has been the case, and this increasing extravagance has only been temporarily checked by recent high prices.

The common practice in house building is to put the fire-grate immediately below and within a chimney; and, as this chimney is formed of brickwork, by no possibility can more than the most minute amount of heat be communicated from the chimney to the room. The main part of the conducted heat of the fire inevitably goes up the chimney, and is wasted, leaving the room to be warmed principally, if not entirely, by the radiated heat. Besides this, it must be remembered that, ordinarily speaking, no provision is made by architects or builders for the proper supply of air to the fire-places, and hence arise smoky chimneys and other evils of the present system. Here, then, is room for much improvement, and we are glad to perceive that the Society of Arts is giving its attention seriously to the matter, and it is to be hoped that some beneficial effects may be the result. As an evidence of how improved efficiency may be combined with economy, in this respect we may refer to Captain Douglas Galton's fire-grate, on which a paper was read before the British Association at Norwich, in 1868. This consists in putting a flue to the upper part of the fire-grate, which flue passes through a brick chamber formed in the ordinary chimney; this chamber being supplied with air from the exterior of the room by a proper channel, and then the air, after being heated in contact with the flue in the chamber, escapes into the room by openings near the ceiling, so that the room is supplied with a copious volume of warm fresh air, thus doing away with all tendency to draughts from the doors and windows, and furnishing an ample supply for the purposes of ventilation and combustion.

VIII. NOTES OF AN ENQUIRY INTO THE PHENOMENA CALLED SPIRITUAL, DURING THE YEARS 1870-73.

By William Crookes, F.R.S., &c.

LIKE a traveller exploring some distant country, the wonders of which have hitherto been known only through reports and rumours of a vague or distorted character, so for four years have I been occupied in pushing an enquiry into a territory of natural knowledge which offers almost virgin soil to a scientific man. As the traveller sees in the natural phenomena he may witness the action of forces governed by natural laws, where others see only the capricious intervention of offended gods, so have I endeavoured to trace the operation of natural laws and forces, where others have seen only the agency of supernatural beings, owning no laws, and obeying no force but their own free will. As the traveller in his wanderings is entirely dependent on the goodwill and friendliness of the chiefs and the medicine men of the tribes amongst whom he sojourns, so have I not only been aided in my enquiry in a marked degree by some of those who possess the peculiar powers I have sought to examine, but have also formed firm and valued friendships amongst many of the recognised leaders of opinion, whose hospitalities I have shared. As the traveller sometimes sends home, when opportunity offers, a brief record of progress, which record, being necessarily isolated from all that has led up to it, is often received with disbelief or ridicule, so have I on two occasions selected and published what seemed to be a few striking and definite *facts*; but having omitted to describe the preliminary stages necessary to lead the public mind up to an appreciation of the phenomena and to show how they fitted into other observed facts, they were also met, not only with incredulity, but with no little abuse. And, lastly, as the traveller, when his exploration is finished and he returns to his old associates, collects together all his scattered notes, tabulates them, and puts them in order ready to be given to the world as a connected narrative, so have I, on reaching this stage of the enquiry, arranged and put together all my disconnected observations ready to place before the public in the form of a volume.

The phenomena I am prepared to attest are so extraordinary and so directly oppose the most firmly rooted

articles of scientific belief—amongst others, the ubiquity and invariable action of the law of gravitation—that, even now, on recalling the details of what I witnessed, there is an antagonism in my mind between *reason*, which pronounces it to be scientifically impossible, and the consciousness that my senses, both of touch and sight,—and these corroborated, as they were, by the senses of all who were present,—are not lying witnesses when they testify against my preconceptions.*

But the supposition that there is a sort of mania or delusion which suddenly attacks a whole roomful of intelligent persons who are quite sane elsewhere, and that they all concur to the minutest particulars, in the details of the occurrences of which they suppose themselves to be witnesses, seems to my mind more incredible than even the facts they attest.

The subject is far more difficult and extensive than it appears. Four years ago I intended only to devote a leisure month or two to ascertain whether certain marvellous occurrences I had heard about would stand the test of close scrutiny. Having, however, soon arrived at the same conclusion as, I may say, every impartial enquirer, that there was "something in it," I could not, as a student of nature's laws, refuse to follow the enquiry wheresoever the facts might lead. Thus a few months have grown into a few years, and were my time at my own disposal it would probably extend still longer. But other matters of scientific and practical interest demand my present attention; and, inasmuch as I cannot afford the time requisite to follow the enquiry as it deserves, and as I am fully confident it will be studied by scientific men a few years hence, and as my opportunities are not now as good as they were some time ago, when Mr. D. D. Home was in good health, and Miss

* The following remarks are so appropriate that I cannot forbear quoting them. They occur in a private letter from an old friend, to whom I had sent an account of some of these occurrences. The high position which he holds in the scientific world renders doubly valuable any opinion he expresses on the mental tendencies of scientific men. "Any *intellectual* reply to your facts I cannot see. Yet it is a curious fact that even I, with all my tendency and desire to believe spiritualistically, and with all my faith in your power of observing and your thorough truthfulness, feel as if I wanted to see for myself; and it is quite painful to me to think how much more proof I want. Painful, I say, because I see that it is not reason which convinces a man, unless a fact is repeated so frequently that the impression becomes like a habit of mind, an old acquaintance, a thing known so long that it cannot be doubted. This is a curious phase of man's mind, and it is remarkably strong in scientific men—stronger than in others, I think. For this reason we must not always call a man dishonest because he does not yield to evidence for a long time. The old wall of belief must be broken down by much battering."

Kate Fox (now Mrs. Jencken) was free from domestic and maternal occupations, I feel compelled to suspend further investigation for the present.

To obtain free access to some persons abundantly endowed with the power I am experimenting upon, now involves more favour than a scientific investigator should be expected to make of it. Spiritualism amongst its more devout followers is a religion. The mediums, in many cases young members of the family, are guarded with a seclusion and jealousy which an outsider can penetrate with difficulty. Being earnest and conscientious believers in the truth of certain doctrines which they hold to be substantiated by what appear to them to be miraculous occurrences, they seem to hold the presence of scientific investigation as a profanation of the shrine. As a personal favour I have more than once been allowed to be present at meetings that presented rather the form of a religious ceremony than of a spiritualistic *séance*. But to be admitted by favour once or twice, as a stranger might be allowed to witness the Eleusinian mysteries, or a Gentile to peep within the Holy of Holies, is not the way to ascertain facts and discover laws. To gratify curiosity is one thing; to carry on systematic research is another. I am seeking the truth continually. On a few occasions, indeed, I have been allowed to apply tests and impose conditions; but only once or twice have I been permitted to carry off the priestess from her shrine, and in my own house, surrounded by my own friends, to enjoy opportunities of testing the phenomena I had witnessed elsewhere under less conclusive conditions.* My observations on these cases will find their due place in the work I am about to publish.

Following the plan adopted on previous occasions,—a plan which, however much it offended the prejudices of some critics, I have good reason to know was acceptable to the readers of the "Quarterly Journal of Science,"—I intended to embody the results of my labour in the form of one or two articles for this journal. However, on going over my notes, I find such a wealth of facts, such a superabundance of evidence, so overwhelming a mass of testimony, all of which will have to be marshalled in order, that I could fill several numbers of the "Quarterly." I must

* In this paper I give no instances and use no arguments drawn from these exceptional cases. Without this explanation it might be thought that the immense number of facts I have accumulated were principally obtained on the few occasions here referred to, and the objection would naturally arise of insufficiency of scrutiny from want of time.

therefore be content on this occasion with an outline only of my labours, leaving proofs and full details to another occasion.

My principal object will be to place on record a series of actual occurrences which have taken place in my own house, in the presence of trustworthy witnesses, and under as strict test conditions as I could devise. Every fact which I have observed is, moreover, corroborated by the records of independent observers at other times and places. It will be seen that the facts are of the most astounding character, and seem utterly irreconcilable with all known theories of modern science. Having satisfied myself of their *truth*, it would be moral cowardice to withhold my testimony because my previous publications were ridiculed by critics and others who knew nothing whatever of the subject, and who were too prejudiced to see and judge for themselves whether or not there was truth in the phenomena; I shall state simply what I have seen and proved by repeated experiment and test, and "I have yet to learn that it is irrational to endeavour to discover the causes of unexplained phenomena."

At the commencement, I must correct one or two errors which have taken firm possession of the public mind. One is that *darkness* is essential to the phenomena. This is by no means the case. Except where darkness has been a necessary condition, as with some of the phenomena of luminous appearances, and in a few other instances, everything recorded has taken place *in the light*. In the few cases where the phenomena noted have occurred in darkness I have been very particular to mention the fact; moreover some special reason can be shown for the exclusion of light, or the results have been produced under such perfect test conditions that the suppression of one of the senses has not really weakened the evidence.

Another common error is that the occurrences can be witnessed only at certain times and places,—in the rooms of the medium, or at hours previously arranged; and arguing from this erroneous supposition, an analogy has been insisted on between the phenomena called spiritual and the feats of legerdemain by professional "conjurors" and "wizards," exhibited on their own platform and surrounded by all the appliances of their art.

To show how far this is from the truth, I need only say that, with very few exceptions, the many hundreds of facts I am prepared to attest,—facts which to imitate by known mechanical or physical means would baffle the skill of a

Houdin, a Bosco, or an Anderson, backed with all the resources of elaborate machinery and the practice of years,—have all taken place in my own house, at times appointed by myself, and under circumstances which absolutely precluded the employment of the very simplest instrumental aids.

A third error is that the medium must select his own circle of friends and associates at a *séance;* that these friends must be thorough believers in the truth of whatever doctrine the medium enunciates; and that *conditions* are imposed on any person present of an investigating turn of mind, which entirely preclude accurate observation and facilitate trickery and deception. In reply to this, I can state that, (with the exception of the very few cases to which I have alluded in a previous paragraph* where, whatever might have been the motive for exclusiveness, it certainly was not the veiling of deception), I have chosen my own circle of friends, have introduced any hard-headed unbeliever whom I pleased, and have generally imposed my own terms, which have been carefully chosen to prevent the possibility of fraud. Having gradually ascertained some of the conditions which facilitate the occurrence of the phenomena, my modes of conducting these inquiries have generally been attended with equal, and, indeed, in most cases with more, success than on other occasions, where, through mistaken notions of the importance of certain trifling observances, the conditions imposed might render less easy the detection of fraud.

I have said that darkness is not essential. It is, however, a well-ascertained fact that when the force is weak a bright light exerts an interfering action on some of the phenomena. The power possessed by Mr. Home is sufficiently strong to withstand this antagonistic influence; consequently, he always objects to darkness at his *séances*. Indeed, except on two occasions, when, for some particular experiments of my own, light was excluded, everything which I have witnessed with him has taken place in the light. I have had many opportunities of testing the action of light of different sources and colours, such as sun-light, diffused day-light, moon-light, gas, lamp, and candle-light, electric light from a vacuum tube, homogeneous yellow light, &c. The interfering rays appear to be those at the extreme end of the spectrum.

I now proceed to classify some of the phenomena which

* See note on page 79.

have come under my notice, proceeding from the simple to the more complex, and briefly giving under each heading an outline of some of the evidence I am prepared to bring forward. My readers will remember that, with the exception of cases specially mentioned, the occurrences have taken place *in my own house, in the light, and with only private friends present* besides the medium. In the contemplated volume I propose to give in full detail the tests and precautions adopted on each occasion, with names of witnesses. I only briefly allude to them in this article.

Class I.

The Movement of Heavy Bodies with Contact, but without Mechanical Exertion.

This is one of the simplest forms of the phenomena observed. It varies in degree from a quivering or vibration of the room and its contents to the actual rising into the air of a heavy body when the hand is placed on it. The retort is obvious that if people are touching a thing when it moves, they push it, or pull it, or lift it; I have proved experimentally that this is not the case in numerous instances, but as a matter of evidence I attach little importance to this class of phenomena by itself, and only mention them as a preliminary to other movements of the same kind, but without contact.

These movements (and indeed I may say the same of every kind of phenomenon) are generally preceded by a peculiar cold air, sometimes amounting to a decided wind. I have had sheets of paper blown about by it, and a thermometer lowered several degrees. On some occasions, which I will subsequently give more in detail, I have not detected any actual movement of the air, but the cold has been so intense that I could only compare it to that felt when the hand has been within a few inches of frozen mercury.

Class II.

The Phenomena of Percussive and other Allied Sounds.

The popular name of "raps" conveys a very erroneous impression of this class of phenomena. At different times, during my experiments, I have heard delicate ticks, as with the point of a pin; a cascade of sharp sounds as from an induction coil in full work; detonations in the air; sharp metallic taps; a cracking like that heard when a frictional machine is at work; sounds like scratching; the twittering as of a bird, &c.

These sounds are noticed with almost every medium, each having a special peculiarity; they are more varied with Mr. Home, but for power and certainty I have met with no one who at all approached Miss Kate Fox. For several months I enjoyed almost unlimited opportunity of testing the various phenomena occurring in the presence of this lady, and I especially examined the phenomena of these sounds. With mediums, generally, it is necessary to sit for a formal *séance* before anything is heard; but in the case of Miss Fox it seems only necessary for her to place her hand on any substance for loud thuds to be heard in it, like a triple pulsation, sometimes loud enough to be heard several rooms off. In this manner I have heard them in a living tree—on a sheet of glass—on a stretched iron wire—on a stretched membrane—a tambourine—on the roof of a cab—and on the floor of a theatre. Moreover, actual contact is not always necessary; I have had these sounds proceeding from the floor, walls, &c., when the medium's hands and feet were held—when she was standing on a chair—when she was suspended in a swing from the ceiling—when she was enclosed in a wire cage—and when she had fallen fainting on a sofa. I have heard them on a glass harmonicon—I have felt them on my own shoulder and under my own hands. I have heard them on a sheet of paper, held between the fingers by a piece of thread passed through one corner. With a full knowledge of the numerous theories which have been started, chiefly in America, to explain these sounds, I have tested them in every way that I could devise, until there has been no escape from the conviction that they were true objective occurrences not produced by trickery or mechanical means.

An important question here forces itself upon the attention. *Are the movements and sounds governed by intelligence?* At a very early stage of the enquiry, it was seen that the power producing the phenomena was not merely a blind force, but was associated with or governed by intelligence: thus the sounds to which I have just alluded will be repeated a definite number of times, they will come loud or faint, and in different places at request; and by a pre-arranged code of signals, questions are answered, and messages given with more or less accuracy.

The intelligence governing the phenomena is sometimes manifestly below that of the medium. It is frequently in direct opposition to the wishes of the medium: when a determination has been expressed to do something which might not be considered quite right, I have known urgent

messages given to induce a reconsideration. The intelligence is sometimes of such a character as to lead to the belief that it does not emanate from any person present.

Several instances can be given to prove each of these statements, but the subject will be more fully discussed subsequently, when treating of the source of the intelligence.

Class III.

The Alteration of Weight of Bodies.

I have repeated the experiments already described in this Journal, in different forms, and with several mediums. I need not further allude to them here.

Class IV.

Movements of Heavy Substances when at a Distance from the Medium.

The instances in which heavy bodies, such as tables, chairs, sofas, &c. have been moved, when the medium has not been touching them, are very numerous. I will briefly mention a few of the most striking. My own chair has been twisted partly round, whilst my feet were off the floor. A chair was seen by all present to move slowly up to the table from a far corner, when all were watching it; on another occasion an arm chair moved to where we were sitting, and then moved slowly back again (a distance of about three feet) at my request. On three successive evenings a small table moved slowly across the room, under conditions which I had specially pre-arranged, so as to answer any objection which might be raised to the evidence. I have had several repetitions of the experiment considered by the Committee of the Dialectical Society to be conclusive, viz., the movement of a heavy table in full light, the chairs turned with their backs to the table, about a foot off, and each person kneeling on his chair, with hands resting over the backs of the chair, but not touching the table. On one occasion this took place when I was moving about so as to see how everyone was placed.

Class V.

The Rising of Tables and Chairs off the Ground, without Contact with any Person.

A remark is generally made when occurrences of this kind are mentioned, Why is it only tables and chairs which do these things? Why is this property peculiar to furniture? I might reply that I only observe and record facts, and do not profess to enter into the Why and Wherefore; but indeed

it will be obvious that if a heavy inanimate body in an ordinary dining-room has to rise off the floor, it cannot very well be anything else but a table or a chair. That this propensity is not specially attached to furniture, I have abundant evidence; but, like other experimental demonstrators, the intelligence or power, whatever it may be, which produces these phenomena can only work with the materials which are available.

On five separate occasions, a heavy dining-table rose between a few inches and 1½ feet off the floor, under special circumstances, which rendered trickery impossible. On another occasion, a heavy table rose from the floor in full light, while I was holding the medium's hands and feet. On another occasion the table rose from the floor, not only when no person was touching it, but under conditions which I had pre-arranged so as to assure unquestionable proof of the fact.

Class VI.

The Levitation of Human Beings.

This has occurred in my presence on four occasions in darkness. The test conditions under which they took place were quite satisfactory, so far as the judgment was concerned; but ocular demonstration of such a fact is so necessary to disturb our pre-formed opinions as to "the naturally possible and impossible," that I will here only mention cases in which the deductions of reason were confirmed by the sense of sight.

On one occasion I witnessed a chair, with a lady sitting on it, rise several inches from the ground. On another occasion, to avoid the suspicion of this being in some way performed by herself, the lady knelt on the chair in such manner that its four feet were visible to us. It then rose about three inches, remained suspended for about ten seconds, and then slowly descended. At another time two children, on separate occasions, rose from the floor with their chairs, in full daylight, under (to me) most satisfactory conditions; for I was kneeling and keeping close watch upon the feet of the chair, and observing that no one might touch them.

The most striking cases of levitation which I have witnessed have been with Mr. Home. On three separate occasions have I seen him raised completely from the floor of the room. Once sitting in an easy chair, once kneeling on his chair, and once standing up. On each occasion I had full opportunity of watching the occurrence as it was taking place.

There are at least a hundred recorded instances of Mr. Home's rising from the ground, in the presence of as many separate persons, and I have heard from the lips of the three witnesses to the most striking occurrence of this kind—the Earl of Dunraven, Lord Lindsay, and Captain C. Wynne—their own most minute accounts of what took place. To reject the recorded evidence on this subject is to reject all human testimony whatever; for no fact in sacred or profane history is supported by a stronger array of proofs.

The accumulated testimony establishing Mr. Home's levitations is overwhelming. It is greatly to be desired that some person, whose evidence would be accepted as conclusive by the scientific world—if indeed there lives a person whose testimony *in favour* of such phenomena would be taken—would seriously and patiently examine these alleged facts. Most of the eye-witnesses to these levitations are now living, and would, doubtless, be willing to give their evidence. But, in a few years, such *direct* evidence will be difficult, if not impossible, to be obtained.

Class VII.

Movement of Various Small Articles without Contact with any Person.

Under this heading I propose to describe some special phenomena which I have witnessed. I can do little more here than allude to some of the more striking facts, all of which, be it remembered, have occurred under circumstances that render trickery impossible. But it is idle to attribute these results to trickery, for I would again remind my readers that what I relate has not been accomplished at the house of a medium, but in my own house, where preparations have been quite impossible. A medium, walking into my dining-room, cannot, while seated in one part of the room with a number of persons keenly watching him, by trickery make an accordion play in *my own* hand when I hold it keys downwards, or cause the same accordion to float about the room playing all the time. He cannot introduce machinery which will wave window-curtains or pull up Venetian blinds 8 feet off, tie a knot in a handkerchief and place it in a far corner of the room, sound notes on a distant piano, cause a card-plate to float about the room, raise a water-bottle and tumbler from the table, make a coral necklace rise on end, cause a fan to move about and fan the company, or set in motion a pendulum when enclosed in a glass case firmly cemented to the wall.

Class VIII.

Luminous Appearances.

These, being rather faint, generally require the room to be darkened. I need scarcely remind my readers again that, under these circumstances, I have taken proper precautions to avoid being imposed upon by phosphorised oil or other means. Moreover, many of these lights are such as I have tried to imitate artificially, but cannot.

Under the strictest test conditions, I have seen a solid self-luminous body, the size and nearly the shape of a turkey's egg, float noiselessly about the room, at one time higher than any one present could reach standing on tiptoe, and then gently descend to the floor. It was visible for more than ten minutes, and before it faded away it struck the table three times with a sound like that of a hard, solid body. During this time the medium was lying back, apparently insensible, in an easy chair.

I have seen luminous points of light darting about and settling on the heads of different persons; I have had questions answered by the flashing of a bright light a desired number of times in front of my face. I have seen sparks of light rising from the table to the ceiling, and again falling upon the table, striking it with an audible sound. I have had an alphabetic communication given by luminous flashes occurring before me in the air, whilst my hand was moving about amongst them. I have seen a luminous cloud floating upwards to a picture. Under the strictest test conditions, I have more than once had a solid, self-luminous, crystalline body placed in my hand by a hand which did not belong to any person in the room. *In the light*, I have seen a luminous cloud hover over a heliotrope on a side table, break a sprig off, and carry the sprig to a lady; and on some occasions I have seen a similar luminous cloud visibly condense to the form of a hand and carry small objects about. These, however, more properly belong to the next class of phenomena.

Class IX.

The Appearance of Hands, either Self-Luminous or Visible by Ordinary Light.

The forms of hands are frequently *felt* at dark *séances*, or under circumstances where they cannot be seen. More rarely I have *seen* the hands. I will here give no instances in which the phenomenon has occurred in darkness, but will

simply select a few of the numerous instances in which I have seen the hands in the light.

A beautifully-formed small hand rose up from an opening in a dining-table and gave me a flower; it appeared and then disappeared three times at intervals, affording me ample opportunity of satisfying myself that it was as real in appearance as my own. This occurred in the light in my own room, whilst I was holding the medium's hands and feet.

On another occasion, a small hand and arm, like a baby's, appeared playing about a lady who was sitting next to me. It then passed to me and patted my arm and pulled my coat several times.

At another time, a finger and thumb were seen to pick the petals from a flower in Mr. Home's button-hole, and lay them in front of several persons who were sitting near him.

A hand has repeatedly been seen by myself and others playing the keys of an accordion, both of the medium's hands being visible at the same time, and sometimes being held by those near him.

The hands and fingers do not always appear to me to be solid and life-like. Sometimes, indeed, they present more the appearance of a nebulous cloud partly condensed into the form of a hand. This is not equally visible to all present. For instance, a flower or other small object is seen to move; one person present will see a luminous cloud hovering over it, another will detect a nebulous-looking hand, whilst others will see nothing at all but the moving flower. I have more than once seen, first an object move, then a luminous cloud appear to form about it, and, lastly, the cloud condense into shape and become a perfectly-formed hand. At this stage, the hand is visible to all present. It is not always a mere form, but sometimes appears perfectly life-like and graceful, the fingers moving and the flesh apparently as human as that of any in the room. At the wrist, or arm, it becomes hazy, and fades off into a luminous cloud.

To the touch, the hand sometimes appears icy cold and dead, at other times, warm and life-like, grasping my own with the firm pressure of an old friend.

I have retained one of these hands in my own, firmly resolved not to let it escape. There was no struggle or effort made to get loose, but it gradually seemed to resolve itself into vapour, and faded in that manner from my grasp.

Class X.

Direct Writing.

This is the term employed to express writing which is not produced by any person present. I have had words and messages repeatedly written on privately-marked paper, under the most rigid test conditions, and have heard the pencil moving over the paper in the dark. The conditions—pre-arranged by myself—have been so strict as to be equally convincing to my mind as if I had seen the written characters formed. But as space will not allow me to enter into full particulars, I will merely select two instances in which my eyes as well as ears were witnesses to the operation.

The first instance which I shall give took place, it is true, at a dark *séance*, but the result was not less satisfactory on that account. I was sitting next to the medium, Miss Fox, the only other persons present being my wife and a lady relative, and I was holding the medium's two hands in one of mine, whilst her feet were resting on my feet. Paper was on the table before us, and my disengaged hand was holding a pencil.

A luminous hand came down from the upper part of the room, and after hovering near me for a few seconds, took the pencil from my hand, rapidly wrote on a sheet of paper, threw the pencil down, and then rose up over our heads, gradually fading into darkness.

My second instance may be considered the record of a failure. "A good failure often teaches more than the most successful experiment." It took place in the light, in my own room, with only a few private friends and Mr. Home present. Several circumstances, to which I need not further allude, had shown that the power that evening was strong. I therefore expressed a wish to witness the actual production of a written message such as I had heard described a short time before by a friend. Immediately an alphabetic communication was made as follows—"We will try." A pencil and some sheets of paper had been lying on the centre of the table; presently the pencil rose up on its point, and after advancing by hesitating jerks to the paper fell down. It then rose and again fell. A third time it tried, but with no better result. After three unsuccessful attempts, a small wooden lath, which was lying near upon the table, slid towards the pencil, and rose a few inches from the table; the pencil rose again, and propping itself against the lath, the two together made an effort to mark the paper. It fell, and then

a joint effort was again made. After a third trial the lath gave it up and moved back to its place, the pencil lay as it fell across the paper, and an alphabetic message told us —"We have tried to do as you asked, but our power is exhausted."

Class XI.

Phantom Forms and Faces.

These are the rarest of the phenomena I have witnessed. The conditions requisite for their appearance appear to be so delicate, and such trifles interfere with their production, that only on very few occasions have I witnessed them under satisfactory test conditions. I will mention two of these cases.

In the dusk of the evening, during a *séance* with Mr. Home at my house, the curtains of a window about eight feet from Mr. Home were seen to move. A dark, shadowy, semi-transparent form, like that of a man, was then seen by all present standing near the window, waving the curtain with his hand. As we looked, the form faded away and the curtains ceased to move.

The following is a still more striking instance. As in the former case, Mr. Home was the medium. A phantom form came from a corner of the room, took an accordion in its hand, and then glided about the room playing the instrument. The form was visible to all present for many minutes, Mr. Home also being seen at the same time. Coming rather close to a lady who was sitting apart from the rest of the company, she gave a slight cry, upon which it vanished.

Class XII.

Special Instances which seem to point to the Agency of an Exterior Intelligence.

It has already been shown that the phenomena are governed by an intelligence. It becomes a question of importance as to the source of that intelligence. Is it the intelligence of the medium, of any of the other persons in the room, or is it an exterior intelligence? Without wishing at present to speak positively on this point, I may say that whilst I have observed many circumstances which appear to show that the will and intelligence of the medium have much to do with the phenomena,* I have observed

* I do not wish my meaning to be misunderstood. What I mean is, *not* that the medium's will and intelligence are actively employed in any conscious or dishonest way in the production of the phenomena, but that they sometimes appear to act in an unconscious manner.

some circumstances which seem conclusively to point to the agency of an outside intelligence, not belonging to any human being in the room. Space does not allow me to give here all the arguments which can be adduced to prove these points, but I will briefly mention one or two circumstances out of many.

I have been present when several phenomena were going on at the same time, some being unknown to the medium. I have been with Miss Fox when she has been writing a message automatically to one person present, whilst a message to another person on another subject was being given alphabetically by means of "raps," and the whole time she was conversing freely with a third person on a subject totally different from either. Perhaps a more striking instance is the following:—

During a *séance* with Mr. Home, a small lath, which I have before mentioned, moved across the table to me, in the light, and delivered a message to me by tapping my hand; I repeating the alphabet, and the lath tapping me at the right letters. The other end of the lath was resting on the table, some distance from Mr. Home's hands.

The taps were so sharp and clear, and the lath was evidently so well under control of the invisible power which was governing its movements, that I said, "Can the intelligence governing the motion of this lath change the character of the movements, and give me a telegraphic message through the Morse alphabet by taps on my hand?" (I have every reason to believe that the Morse code was quite unknown to any other person present, and it was only imperfectly known to me). Immediately I said this, the character of the taps changed, and the message was continued in the way I had requested. The letters were given too rapidly for me to do more than catch a word here and there, and consequently I lost the message; but I heard sufficient to convince me that there was a good Morse operator at the other end of the line, wherever that might be.

Another instance. A lady was writing automatically by means of the planchette. I was trying to devise a means of proving that what she wrote was not due to "unconscious cerebration." The planchette, as it always does, insisted that, although it was moved by the hand and arm of the lady, the *intelligence* was that of an invisible being who was playing on her brain as on a musical instrument, and thus moving her muscles. I therefore said to this intelligence, "Can you see the contents of this room?"

"Yes," wrote the planchette. "Can you see to read this newspaper?" said I, putting my finger on a copy of the *Times*, which was on a table behind me, but without looking at it. "Yes" was the reply of the planchette. "Well," I said, "if you can see that, write the word which is now covered by my finger, and I will believe you." The planchette commenced to move. Slowly and with great difficulty, the word "however" was written. I turned round and saw that the word "however" was covered by the tip of my finger.

I had purposely avoided looking at the newspaper when I tried this experiment, and it was impossible for the lady, had she tried, to have seen any of the printed words, for she was sitting at one table, and the paper was on another table behind, my body intervening.

Class XIII.

Miscellaneous Occurrences of a Complex Character.

Under this heading I propose to give several occurrences which cannot be otherwise classified owing to their complex character. Out of more than a dozen cases, I will select two. The first occurred in the presence of Miss Kate Fox. To render it intelligible, I must enter into some details.

Miss Fox had promised to give me a *séance* at my house one evening in the spring of last year. Whilst waiting for her, a lady relative, with my two eldest sons, aged fourteen and eleven, were sitting in the dining-room where the *séances* were always held, and I was sitting by myself, writing in the library. Hearing a cab drive up and the bell ring, I opened the door to Miss Fox, and took her directly into the dining-room. She said she would not go upstairs, as she could not stay very long, but laid her bonnet and shawl on a chair in the room. I then went to the dining-room door, and telling the two boys to go into the library and proceed with their lessons, I closed the door behind them, locked it, and (according to my usual custom at *séances*) put the key in my pocket.

We sat down, Miss Fox being on my right hand and the other lady on my left. An alphabetic message was soon given to turn the gas out, and we thereupon sat in total darkness, I holding Miss Fox's two hands in one of mine the whole time. Very soon, a message was given in the following words, "We are going to bring something to show our power;" and almost immediately afterwards, we all heard the tinkling of a bell, not stationary, but moving about in

all parts of the room; at one time by the wall, at another in a further corner of the room, now touching me on the head, and now tapping against the floor. After ringing about the room in this manner for fully five minutes, it fell upon the table close to my hands.

During the time this was going on, no one moved and Miss Fox's hands were perfectly quiet. I remarked that it could not be my little hand-bell which was ringing, for I left that in the library. (Shortly before Miss Fox came, I had occasion to refer to a book, which was lying on a corner of a book-shelf. The bell was on the book, and I put it on one side to get the book. That little incident had impressed on my mind the fact of the bell being in the library.) The gas was burning brightly in the hall outside the dining-room door, so that this could not be opened without letting light into the room, even had there been an accomplice in the house with a duplicate key, which there certainly was not.

I struck a light. There, sure enough, was my own bell lying on the table before me. I went straight into the library. A glance showed that the bell was not where it ought to have been. I said to my eldest boy, "Do you know where my little bell is?" "Yes, papa," he replied, "there it is," pointing to where I had left it. He looked up as he said this, and then continued, "No—it's not there, but it was there a little time ago." "How do you mean?—has anyone come in and taken it?" "No," said he, "no one has been in; but I am sure it was there, because when you sent us in here out of the dining-room, J. (the youngest boy) began ringing it so that I could not go on with my lessons, and I told him to stop." J. corroborated this, and said that, after ringing it, he put the bell down where he had found it.

The second circumstance which I will relate occurred in the light, one Sunday evening, only Mr. Home and members of my family being present. My wife and I had been spending the day in the country, and had brought home a few flowers we had gathered. On reaching home, we gave them to a servant to put them in water. Mr. Home came soon after, and we at once proceeded to the dining-room. As we were sitting down, a servant brought in the flowers which she had arranged in a vase. I placed it in the centre of the dining-table, which was without a cloth. This was the first time Mr. Home had seen these flowers.

After several phenomena had occurred, the conversation turned upon some circumstances which seemed only explicable on the assumption that matter had actually passed

through a solid substance. Thereupon a message was given by means of the alphabet: "It is impossible for matter to pass through matter, but we will show you what we can do." We waited in silence. Presently a luminous appearance was seen hovering over the bouquet of flowers, and then, in full view of all present, a piece of china-grass 15 inches long, which formed the centre ornament of the bouquet, slowly rose from the other flowers, and then descended to the table in front of the vase between it and Mr. Home. It did not stop on reaching the table, but went straight through it, and we all watched it till it had entirely passed through. Immediately on the disappearance of the grass, my wife, who was sitting near Mr. Home, saw a hand come up from under the table between them, holding the piece of grass. It tapped her on the shoulder two or three times with a sound audible to all, then laid the grass on the floor, and disappeared. Only two persons saw the hand, but all in the room saw the piece of grass moving about as I have described. During the time this was taking place, Mr. Home's hands were seen by all to be quietly resting on the table in front of him. The place where the grass disappeared was 18 inches from his hands. The table was a telescope dining-table, opening with a screw; there was no leaf in it, and the junction of the two sides formed a narrow crack down the middle. The grass had passed through this chink, which I measured, and found to be barely $\frac{1}{8}$th inch wide. The stem of the piece of grass was far too thick to enable me to force it through this crack without injuring it, yet we had all seen it pass through quietly and smoothly; and on examination, it did not show the slightest signs of pressure or abrasion.

Theories to Account for the Phenomena Observed.

First Theory.—The phenomena are all the results of tricks, clever mechanical arrangements, or legerdemain; the mediums are impostors, and the rest of the company fools.

It is obvious that this theory can only account for a very small proportion of the facts observed. I am willing to admit that some so-called mediums of whom the public have heard much are arrant impostors who have taken advantage of the public demand for spiritualistic excitement to fill their purses with easily earned guineas; whilst others who have no pecuniary motive for imposture are tempted to cheat, it would seem, solely by a desire for notoriety. I have met with several cases of imposture, some very ingenious, others so palpable, that no person who has witnessed the

genuine phenomena could be taken in by them. An enquirer into the subject finding one of these cases at his first initiation is disgusted with what he detects at once to be an imposture; and he not unnaturally gives vent to his feelings, privately or in print, by a sweeping denunciation of the whole genus "medium." Again, with a thoroughly genuine medium, the first phenomena which are observed are generally slight movements of the table, and faint taps under the medium's hands or feet. These of course are quite easy to be imitated by the medium, or anyone at the table. If, as sometimes occurs, nothing else takes place, the sceptical observer goes away with the firm impression that his superior acuteness detected cheating on the part of the medium, who was consequently afraid to proceed with any more tricks in *his* presence. He, too, writes to the newspapers exposing the whole imposture, and probably indulges in moral sentiments about the sad spectacle of persons, apparently intelligent, being taken in by imposture which he detected at once.

There is a wide difference between the tricks of a professional conjurer, surrounded by his apparatus, and aided by any number of concealed assistants and confederates, deceiving the senses by clever sleight of hand on his own platform, and the phenomena occurring in the presence of Mr. Home, which take place in the light, in a private room that almost up to the commencement of the *séance* has been occupied as a living room, and surrounded by private friends of my own, who not only will not countenance the slightest deception, but who are watching narrowly every thing that takes place. Moreover, Mr. Home has frequently been searched before and after the *séances*, and he *always* offers to allow it. During the most remarkable occurrences I have occasionally held both his hands, and placed my feet on his feet. On no single occasion have I proposed a modification of arrangements for the purpose of rendering trickery less possible which he has not at once assented to, and frequently he has himself drawn attention to tests which might be tried.

I speak chiefly of Mr. Home, as he is so much more powerful than most of the other mediums I have experimented with. But with all I have taken such precautions as place trickery out of the list of possible explanations.

Be it remembered that an explanation to be of any value must satisfy *all* the conditions of the problem. It is not enough for a person, who has perhaps seen only a few of the inferior phenomena, to say "I suspect it was all cheating," or, "I saw how some of the tricks could be done."

Second Theory.—The persons at a *séance* are the victims of a sort of mania or delusion, and imagine phenomena to occur which have no real objective existence.

Third Theory.—The whole is the result of conscious or unconscious cerebral action.

These two theories are evidently incapable of embracing more than a small portion of the phenomena, and they are improbable explanations for even those. They may be dismissed very briefly.

I now approach the "Spiritual" theories. It must be remembered that the word "spirits" is used in a very vague sense by the generality of people.

Fourth Theory.—The result of the spirit of the medium, perhaps in association with the spirits of some or all of the people present.

Fifth Theory.—The actions of evil spirits or devils, personifying who or what they please, in order to undermine Christianity and ruin men's souls.

Sixth Theory.—The actions of a separate order of beings, living on this earth, but invisible and immaterial to us. Able, however, occasionally to manifest their presence. Known in almost all countries and ages as demons (not necessarily bad), gnomes, fairies, kobolds, elves, goblins, Puck, &c.

Seventh Theory.—The actions of departed human beings—the spiritual theory *par excellence.*

Eighth Theory.—(*The Psychic Force Theory*).—This is a necessary adjunct to the 4th, 5th, 6th, and 7th theories, rather than a theory by itself.

According to this theory the "medium," or the circle of people associated together as a whole, is supposed to possess a force, power, influence, virtue, or gift, by means of which intelligent beings are enabled to produce the phenomena observed. What these intelligent beings are is a subject for other theories.

It is obvious that a "medium" possesses a *something* which is not possessed by an ordinary being. Give this *something* a name. Call it "*x*" if you like. Mr. Serjeant Cox calls it Psychic Force. There has been so much misunderstanding on this subject that I think it best to give the following explanation in Mr. Serjeant Cox's own words :—

"The Theory of *Psychic Force* is in itself merely the recognition of the now almost undisputed fact that under certain conditions, as yet but imperfectly ascertained, and within a limited, but as yet undefined, distance from the bodies of certain persons having a special nerve organisation, a Force operates by which, without muscular contact or connection,

action at a distance is caused, and visible motions and audible sounds are produced in solid substances. As the presence of such an organisation is necessary to the phenomenon, it is reasonably concluded that the Force does, in some manner as yet unknown, proceed from that organisation. As the organism is itself moved and directed within its structure by a Force which either is, or is controlled by, the Soul, Spirit, or Mind (call it what we may) which constitutes the individual being we term 'the Man,' it is anequally reasonable conclusion that the Force which causes the motions beyond the limits of the body is the same Force that produces motion within the limits of the body. And, inasmuch as the external force is seen to be often directed by Intelligence, it is an equally reasonable conclusion that the directing Intelligence of the external force is the same Intelligence that directs the Force internally. This is the force to which the name of *Psychic Force* has been given by me as properly designating a force which I thus contend to be traced back to the Soul or Mind of the Man as its source. But I, and all who adopt this theory of Psychic Force as being the agent through which the phenomena are produced, do not thereby intend to assert that this Psychic Force may not be sometimes seized and directed by some other Intelligence than the Mind of the Psychic. The most ardent Spiritualists practically admit the existence of Psychic Force under the very inappropriate name of Magnetism (to which it has no affinity whatever), for they assert that the Spirits of the Dead can only do the acts attributed to them by using the Magnetism (that is, the Psychic Force) of the Medium. The difference between the advocates of Psychic Force and the Spiritualists consists in this—that we contend that there is as yet insufficient proof of any other directing agent than the Intelligence of the Medium, and no proof whatever of the agency of Spirits of the Dead; while the Spiritualists hold it as a faith, not demanding further proof, that Spirits of the Dead are the sole agents in the production of all the phenomena. Thus the controversy resolves itself into a pure question of *fact*, only to be determined by a laborious and long-continued series of experiments and an extensive collection of psychological *facts*, which should be the first duty of the Psychological Society, the formation of which is now in progress."

NOTICES OF BOOKS.

Lectures on Light. Delivered in America in 1872-1873. By JOHN TYNDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. London: Longmans, Green, and Co. 1873. 8vo., 268 pp.

WE are all more or less familiar with the history of Dr. Tyndall's recent visit to America. We know that he was invited to that country to give a course of lectures on Natural Philosophy in the principal cities of the States, and that he was received everywhere with open arms. The thirst of the Americans for science is prodigious; a considerable scientific taste and literature is springing up amongst them; their enterprise induces them to constantly reprint our large works on science; and it will be remembered that these very lectures of Dr. Tyndall's were printed in broadsides, illustrated, and issued, in a newspaper-like form, at the cost of a few cents. The subject chosen was Light, and, in the work before us, we have the course of six lectures on that subject, which Dr. Tyndall delivered in the principle centres of American thought and progress.

Dr. Tyndall has adopted the plan of giving a history of the Science of Light, and illustrating each fact as it was discovered. Thus early in the first lecture we find an account of the law, that the angle of incidence is equal to the angle of reflection; of Snell's Law of the Refraction of Light (1621), sometimes called 'Descartes' Law;' and of Rœmer's Determination of the Velocity of Light (1676). "Snell's Law of Refracton," says our author, "is one of the corner stones of optical science, and its applications to-day are millionfold. Immediately after its discovery, Descartes applied it to the explanation of the rainbow. A beam of solar light falling obliquely upon a raindrop is refracted on entering the drop, and, on emerging, is again refracted." Here follows an explanation of the rainbow, and the means by which Descartes proved its origin. Next we have an account of Newton's Discovery of the Decomposition and Recomposition of Light, and many illustrations of the discovery; then of Achromatism, and the Theory of Colours.

In the second lecture we are introduced to the once rival conjectures concerning the nature of light:—The "Emission Theory," and the "Undulatory Theory;" and here Dr. Tyndall introduces some very pertinent remarks regarding the conception of a physical theory, and the necessary use of the imagination in that form of conception:—"This conception of physical theory implies, as you perceive, the exercise of the imagination. Do not be afraid of this word, which seems to render so many respectable people, both in the ranks of science and out of them,

uncomfortable. That men in the ranks of science should feel thus is, I think, a proof that they have suffered themselves to be misled by the popular definition of a great faculty, instead of observing its operation in their own minds. Without imagination we cannot take a step beyond the bourne of the mere animal world, perhaps not even to the edge of this. But, in speaking thus of imagination, I do not mean a riotous power which deals capriciously with facts, but a well ordered and disciplined power, whose sole function is to form conceptions which the intellect imperatively demands. Imagination thus exercised never really severs itself from the world of fact. This is the storehouse from which all its pictures are drawn; and the magic of its art consists, not in creating things anew, but in so changing the magnitude, position, and other relations of sensible things, as to render them fit for the requirements of the intellect in the subsensible world." The growth of the rival theories is then traced, and the use of imagination by those who favoured one or the other; we were unaware that Sir David Brewster did not adopt the undulatory theory, and certainly unaware that he permitted sentiment to interfere with his acceptation of a physical theory, as the following passage indicates:—"In one of my latest conversations with Sir David Brewster, he said to me that his chief objection to the undulatory theory of light was, that he could not think the Creator guilty of so clumsy a contrivance as the filling of space with ether in order to produce light. This, I may say, is very dangerous ground, and the quarrel of science with Sir David on this point, as with many estimable persons on other points, is, that they profess to know too much about the mind of the Creator." The elaborate and most due praise which Tyndall, and Helmholtz, and many others have bestowed upon Thomas Young here reappears. But when Dr. Tyndall tells us that Young was but a little lower in the intellectual scale than Newton, and greater than any man of science between Newton's time and his own, we cannot, with all possible love for our country and countrymen, hold with him. For he practically makes Newton the greatest man of science of all time, and Young second only to the greatest. Elsewhere he says "we trace the progress of astronomy through Hipparchus and Ptolemy; and after a long halt, through Copernicus, Galileo, Tycho Brahe, and Kepler; while from the high table-land of thought raised by these men Newton shoots upward like a peak, overlooking all others from his dominant elevation." And we would invite comparison between the great and glorious Leonardo da Vinci and the great and glorious Dr. Thomas Young—men who had many remarkable points of contact. If no more, at least they should be "bracketed together" in the list of illustrious men, but we incline to the greater exaltation of Leonardo da Vinci.

The third lecture treats of the double refraction and polarisation of light. We may particularly call attention to Fig. 27, and the

admirably clear explanation which is given of the cause of refraction by a prism, and of the change of wave-front. The polarised beam of light is compared, in the matter of its two-sidedness with the two-endedness of a magnet. The subject of polarisation, and the chromatic phenomena which accompany it, is continued in the fourth lecture; and the many admirable experimental illustrations with which Dr. Tyndall has made us familiar at the Royal Institution are freely introduced to develope the subject more clearly.

The fifth lecture takes us into Dr. Tyndall's more special domain of radiant heat, and the consideration of its relationship to light. The subject of Fluorescence is fully discussed, and the usual solution of sulphate of quinine, and the recently discovered *Thallene* of Prof. Morton used to illustrate it. Then we have an interesting paragraph relating to what is now called "Potential Light." "Fluor-spar, and some other substances, when raised to a temperature still under redness, emit light. During the ages which have elapsed since their formation, this capacity of shaking the ether into visual tremors appears to have been enjoyed by these substances. Light has been potential within them all this time; and, as well explained by Draper, the heat, though not itself of visual intensity, can unlock the molecules so as to enable them to exert the power of vibration which they possess." The observations of Dr. Bence Jones appear to have proved the existence of a fluorescent substance in the human body, notably in the lens of the eye. Thus, if the eye be plunged into the ultra-violet rays, it becomes conscious of a "whitish-blue shimmer" filling the space before it; and, according to Dr. Tyndall, the crystalline lens of the eye, if it be then viewed from without, is seen to gleam vividly. An account of *calorescence*, and of the effects of the non-luminous ultra-red rays is next given, and, near the end of the lecture, of the refraction, polarisation, and magnetisation of heat. We cannot completely understand the connection between the ultra-red rays and certain phenomena in Nature (described on pp. 176 and 177, and could almost imagine that a page or paragraph had been omitted. We can find nothing in the text to prove that the invisible ultra-red rays produce the warming and consequent evaporation of the tropical oceans, and hence, indirectly, our rains and snows. Again, a large flask containing a freezing mixture, and coated with hoar-frost, was placed at an intensely luminous focus of electric light, an alum-cell being interposed; the frost was not melted: when, however, an iodine-cell, which cut off all the light, was interposed, and the alum-cell withdrawn, the hoar-frost was melted. "Hence," says our author, "we infer that the snow and ice which feed the Rhone, the Rhine, and other rivers which have glaciers for their sources, are released from their imprisonment upon the mountains by the invisible ultra-red rays of the sun." Why *hence?* The experiment

indeed proves, that if you cut off all the heat rays, the luminous rays possess no warmth; while, if you cut off all the luminous rays, the heat rays exercise great power; but it does not prove to us that luminous heat plays no part in the phenomena of Nature. A word or two of amplification, which considerably simplify this part of the lecture.

The sixth and final lecture is devoted to the important and rapidly growing subject of Spectrum Analysis, to which we need not very specially refer. This lecture is terminated by no less than nineteen pages of summary and conclusion. From this we may with advantage note here and there, of necessity somewhat desultorily, a remark or a generalisation. Dr. Tyndall speaks of Newton's emission theory in the following terms: "For a century it stood like a dam across the course of discovery; but, like all barriers that rest upon authority and not upon truth, the pressure from behind increased, and swept the barrier away." Of the undulatory theory, he says, "It had been enunciated by Hooke, it had been applied by Huyghens, it had been defended by Euler. But they made no impression. . . . . It first took the form of a demonstrated verity in the hands of Thomas Young. . . . After him came Fresnel, whose transcendent mathematical abilities enabled him to give the theory a generality unattained by Young. He grasped the theory in its entirety; followed the ether into the hearts of crystals of the most complicated structure, and into bodies subjected to strain and pressure. He showed that the facts discovered by Malus, Arago, Brewster, and Biot, were so many ganglia, so to speak, of his theoretic organism, deriving from it sustenance and explanation."

In his concluding remarks, Dr. Tyndall has addressed to the Americans some admirable remarks concerning the so-called practical scientific man and the original investigator. He has begged them not to confound the two, not, as is too often the case, to attribute the discoveries of the original worker to the man who applies them to the practical good of mankind. And he has besought them to endeavour to foster and cultivate original research. "Your most difficult problem will be not to build institutions, but to discover men. You may erect laboratories and endow them, you may furnish them with all the appliances needed for enquiry; in so doing you are but creating opportunity for the exercise of powers which come from sources entirely beyond your reach. You cannot create genius by bidding for it. . . . . . You have scientific genius amongst you—not sown broadcast, believe me, it is sown thus nowhere—but still scattered here and there. Take all unnecessary impediments out of its way. Keep your sympathetic eye upon the originator of knowledge. Give him the freedom necessary for his researches, not overloading him either with the duties of tuition or of administration, not demanding from him so-called practical

results—above all things avoiding that question which ignorance so often addresses to genius, 'What is the use of your work?' Let him make truth his object, however unpractical for the time being, that truth may appear. If you cast your bread thus upon the waters, then be assured it will return to you, though it may be after many days."

We trust our American cousins, or, as we should prefer to call them "brothers, speaking the same dear mother tongue," will lay all this to heart, and let it bear good fruit. We rejoice to know that one of our own scientific men has been received by the Americans, as Dr. Tyndall has been received, and we trust that the establishment of these social relationships will do much to bind together the two great countries into still closer union.

The Spectroscope and its Applications. By J. NORMAN LOCKYER, F.R.S. Macmillan and Co. London: 1873. 8vo., 117 pp. Illustrated.

THIS is the first volume of a series of popular works on Science, to be called the *Nature Series*, because the subject-matter is first printed in "Nature." Eight of these books are already announced, and some two or three will no doubt be ready by next October. The design is good, and the books will probably supply a want which is being felt in this country and America. Judging from the present volume, the series will resemble Messrs. Hachette's *Librarie des Merveilles* more closely than any other works in our language. The general appearance, as to externals, is altogether prepossessing—the book is well printed on thick paper, profusely illustrated, and very neatly bound.

The present volume consists of three lectures on the Spectroscope, delivered before the Society of Arts in 1869. They here appear in a revived and somewhat expanded form, and the subject has, as far as possible, been brought up to the day of issue.

The first lecture regards the matter from an historical and descriptive point of view. The broad points of interest connected with the history of the spectroscope are clearly discussed. The proof that lights which differ in colour differ in refrangibility; that the light of the sun consists of rays possesssing different refrangibilities; the decomposition and recomposition of light. Newton, in his experiments, had used a round hole in a shutter for the admission of a beam of light; while Wollaston, in 1802, made what our author calls "a tremendous step in advance," by substituting a slit instead of a circular hole. This simple modification of Newton's experiment permitted a spectrum of considerable purity to be obtained for the colours, instead of overlapping, were now seen more distinctly side by side, and with only their edges overlapping. In this spectrum Wollaston found breaks of continuity, not observed by Newton; he discovered the black lines at right angles to the length of the

spectrum, which we now call "Fraünhofer's lines." Ten years later, in 1812, the German optician, Fraünhofer mapped no less than 576 of these lines, and lettered the principal ones A, B, C, D; he discovered, moreover, that in the spectrum of certain stars, the black lines do not hold by any means the same position which they hold in the spectrum of the sun. In 1830, the next great improvement in the spectroscope was made. In addition to the simple prism and slit, Mr. Simms, the optician, placed a lens in front of the prism and another lens near the eye, so as to magnify the spectrum. By this means the black lines could be studied with far greater readiness than by the naked eye. "You may imagine the enormous mystery—the wonderful reverence almost—with which this question of the Fraünhofer's lines was approached until they were thoroughly understood; and recollect that we owe the discovery of them, by which we are enabled now to determine the pressures acting in the atmospheres of the most distant stars, simply to the fact that Dr. Wollaston, instead of drilling a round hole, used a slit; and to the other additional fact, that Mr. Simms, instead of using that slit with a mere prism, used a lens, and made the beam parallel, and then allowed that parallel beam, after it had passed through the prism, to pass into another telescope, and form an image of the slit for each ray. You see how closely connected are the grandest discoveries with the skill and suggestiveness of those who supply different instruments for our use." Thus the principal incidents in the history of the spectroscope, as an instrument, are (α) Newton's application of the prism to optical purposes, in 1675; (β) his observation that the prism should be used at the angle of minimum deviation; (γ) the addition of the slit by Wollaston in 1812; (δ) the collimating lens added by Simms in 1830. The author next describes various forms of spectroscopes with one, two, or more prisms. Capital figures are given of Steinheil's four-prism spectroscope, used by Kirchhoff; of Huggins's star spectroscope, and of direct vision spectroscopes with three or four prisms.

The second lecture treats of the applications of the spectroscope, specially of those which depend upon the investigation of light radiated from bodies. Thus applied, the instrument enables us to distinguish between solids, liquids, and gases; and between gases and vapours existing at different pressures. The various methods of obtaining spectra are here discussed; the use of the Bunsen burner, the induction coil, and the voltaic arc. A coloured plate at the commencement of the volume shows, among other things, two spectra of great interest; the one of hydrogen at a high pressure, the other of hydrogen at a low pressure. It is here seen that, in the former instance, the spectrum is much more continuous than in the latter. In fact, in the vacuous tube, the hydrogen spectrum dwindles down to one or two very sharply defined and thin lines. On the other hand, Frankland

has proved that the spectrum of incandescent hydrogen existing under very great pressure is entirely continuous. For intermediate pressures we have intermediate spectra. "We may state generally," says the author, "that beginning with any one element in its most rarefied condition, and then following its spectrum, as the molecules come nearer together, so as at last to reach the solid form, we shall find that spectrum become more and more complicated as this approach takes place, until at last a vivid continuous spectrum is reached." The latter part of this lecture treats of the application of the spectroscope to the investigation of the nature of various heavenly bodies, the sun, stars, and nebulæ, at the hands of various physicists and astronomers. The third and concluding lecture mainly discusses absorption-spectra. It is here shown that it easy to detect different substances by noticing their absorption; by introducing the substance between the source of light and the prism, and noticing the resulting spectrum, side by side with a continuous spectrum. We may discriminate between two solutions, the one of blood, the other of magenta dye, exactly similar in colour and general appearance. Again, Mr. Sorby has proved that, by means of the spectrum-microscope, a blood spot so small that it contains only one-thousandth of a grain of blood may be detected. Some very interesting and comparatively new matter is given near the end of this lecture relating to the probably constitution of sun spots, the spectroscopic examination of which seems to prove that they are due to "*general* absorption, plus *special* absorption in some particular lines." The observed widening of the sodium line in the spectrum of a sun spot is traced to difference of pressure:—"If we take a tube containing some metallic sodium sealed up in hydrogen, and pass a beam of light from the electric lamp through it, by decomposing this beam with our prisms we shall obtain an ordinary continuous spectrum without either bright or dark lines; but by heating the metallic sodium in the tube, which is placed in front of the slit, we really fill that tube with the vapour of sodium; and as the heating will be slow, the sodium vapour will rise very gently from the metal at the bottom, so that we shall get layers of different densities of sodium vapour filling the tube. Immediately the sodium begins to rise in vapour, a black absorption-line shows itself, in one spectrum, in precisely the same position as the yellow line of sodium, and you will find that the thickness of the sodium absorption line will vary with the density of the stratum of vapour through which it passes. Thus, from the upper part of the tube we obtain a fine delicate line, which gradually thickens as we approach the bottom of the tube, and thus we produce the appearance in the spectrum of the spot where the layers of sodium vapour are very dense, and the very fine delicate line of the sodium vapour when thrown up into the sun's chromosphere." Thus, in fact, it seems to be undoubted

that, just as the hydrogen line widens as the surface of the sun is approached, indicating increase of pressure, so, in the sun-spot, the pressure of the incandescent sodium vapour increases as we go deeper into the sun-spot cavity. Finally we have an account of the velocity of solar storms, calculated from the shifting of the F line, to be something like a hundred miles in a second.

Altogether the work is very readable, and it will form a useful introduction to larger works, such as that of Roscoe or Schellen. It shows, however, frequent evidence of haste, both in style of composition and in arrangement of subject-matter, and several somewhat abstruse subjects receive rather slight treatment. We regret also to notice the comparatively slight mention of foreign investigators, and of our own Miller, Brewster, and Herschel. But we must bear in mind that the subject is vast, and three hours of exposition are very insufficient for a detailed account of only one branch of it. We commend the work to all who are interested in one of the most prominent branches of optical science of our day, and remind them that it is the work of one who has done good service to science by his own researches.

Light Science for Leisure Hours. Second Series. Familiar Essays on Scientific Subjects, Natural Phenomena, &c., with a Sketch of the Life of Mary Somerville. By RICHARD A. PROCTOR, B.A., Cambridge, Honorary Secretary of the Royal Astronomical Society, &c. London: Longmans, Green, and Co. 1873.

SOME years ago a caricature portrait of Alexandre Dumas was published in Paris, in which the brilliant novelist was pictured with a bunch of hands attached to each arm, and each hand carrying a pen that was scribbling furiously. The flood of books, magazine articles, controversial and other correspondence, besides communications and hard secretary's work to the Royal Astronomical Society, which, during the last few years, have poured from the pen of Mr. Proctor, render a similar caricature almost justifiable, but it demands some modification. Instead of merely a bunch of supplementary hands, the artist would, in this case, require to depict an efflorescence of supplementary brains, Mr. Proctor's work having been something considerably beyond the efforts of a mere book-maker. He has specially attacked the grandest and heaviest of the sciences, and made it the leading subject of his popular teaching; but, instead of re-baking the old dishes of his predecessors, he has laid before his readers the most recently discovered facts, generalisations, and speculations of modern astronomy; a large proportion of which, according to the old method of dishing up astronomical handbooks, would have remained during another generation or so buried in the Transactions of learned societies, or in the unread volumes

of those who might be so simple-minded as to publish, on their own account, important contributions to astronomical science, under the delusion that they would be studied during their own lifetime.

Mr. Proctor has the high merit—very rare among avowedly popular teachers—of digging, with his own hands, into these depths of astronomical literature, and of directly presenting to readers of all classes judiciously selected and well displayed examples of their treasures.

The second series of "Light Science for Leisure Hours" is one of the latest of these collections of nuggets (that is up to this moment of writing; we cannot tell what may happen during the short time that will elapse before this is published), and is fully equal in interest and value to its predecessors.

A considerable portion of the volume is devoted to meteorological problems, and the interminable "Gulf Stream" discussion, into the lists of which tournament Mr. Proctor has valiantly entered, the device on his shield being surface evaporation over large tropical areas. He repeats Maury's demonstration of the insufficiency of Herschel's trade wind explanation, and Herschel's refutation of Maury's and Humboldt's variation of the specific gravity theory, and then proceeds to show that Dr. Carpenter's lump of ice, at one end of a rectangular aquarium trough, is a fallacious representation of the arctic ice in arctic waters, inasmuch as the arctic area is so much smaller than the tropical that the trough should have been V-shaped, with the ice at the angle, in order to be at all representative. There can be no doubt that this simple quantitative difficulty is fatal to Dr. Carpenter's large estimate of the potency of the arctic ice-cold stream.

Taking a cool outside view of this controversy, it presents one very interesting feature, namely that each combatant succeeds in refuting the *sufficiency* of his opponents explanation, but (as far as those above-named are concerned) all have failed to refute its *actuality*. Hence we may venture to conclude that the errors on all sides are quantitative only, and that the actual oceanic circulation is due to the combined, or rather co-operating action, of all the forces on behalf of which the champions are respectively combating. We say no more, lest the infection of the fight should come upon us and deform our critical impartiality.

The other essays are on "The Coming Transit of Venus" of course, "The Ever Widening World of Stars," Movements in the Star Depths," "The Great Nebula in Orion," "The Sun's True Atmosphere," "Something Wrong with the Sun," "News from Herschel's Planet," "The Two Comets of 1868," "Comets of Short Period," "The Climate of Great Britain," "The Low Barometer of the Antarctic Temperate Zone."

We cannot of course describe or discuss the contents of such

a series. They are all written with Mr. Proctor's usual graphic clearness, and carry the reader forward with the latest steps in the progress of those departments of science which they treat.

Besides these, there is a critical sketch of the life and works of Mrs. Somerville.

In his preface Mr. Proctor invites special attention to the essay on the Transit of Venus, and expresses his practical conclusion on this subject in unmistakable and uncompromising terms, in italics, thus, that "there is great risk that, for want of an adequate number of southern stations, *the whole series of observations, by all countries engaged in the work, will result in failure*," and he adds in a note that, "since this was written, I have received letters from the greatest master of mathematical astronomy this country has produced since Newton's day, strongly confirming my views as to the extreme importance of providing many southern stations for applying Halley's method in 1874, and urging me, moreover, to appeal to America to take part in this special work, for which she is peculiarly fitted, because of the bravery and enterprise of her seamen, the skill and ingenuity of her astronomers and physicists, and her singular liberality as a nation in all scientific matters."

Electricity and Magnetism. By FLEEMING JENKIN, F.R.SS. L. & E., M.I.C.E., Professor of Engineering in the University of Edinburgh. (Text-Books of Science.) London: Longmans, Green, and Co. 1873.

OF the great and increasing value of the series of text-books published by Messrs. Longmans, there cannot be the remotest doubt. Not only are the contents of each little work valuable to the student, but it becomes a pleasure to the proficient to see the principles of his science advanced so clearly and cleverly by the best expounders. Professor Fleeming Jenkin has done ably by electrical science in the above volume of the series; and particularly does he make clear the difficult subject of contact electricity. We commend the work to the notice of our readers interested in electrical science.

Quantitative Chemical Analysis. By T. E. THORPE, Ph., D., F.R.S.E. Professor of Chemistry, Andersonian Institution, Glasgow. London: Longmans, Green, and Co.

THIS work forms another of the "series of text-books of science, adapted for the use of artisans and students in public and other schools." Its speciality is that the "examples chosen" have

been selected to a great extent "on account of their practical utility." In other words, the work has a technological character, which, without unfitting it for students of other classes, makes it particularly suited for artisans. It is divided into five sections. The first of these is a full and carefully compiled treatise on chemical manipulation, as far as qualitative analysis is concerned. The instructions on the use of the balance are admirable, and it would not be easy to point out any necessary precaution which the author has omitted to press on the attention of his readers. Indeed it might be suggested that mention of the method of weighing by vibrations, with the accompanying mathematical formulæ, are not somewhat out of place in such a work. There is also an attempt at determining by algebraical calculations the amount of wash-water, and the minimum number of washings required to bring any precipitate to a state of purity. The use of the filter-pump is explained at full length. The action of the solvents employed upon the apparatus in digestions, evaporations, &c., is pointed out as a possible, though generally overlooked, source of error. In short, not merely students—in the common sense of the term—but not a few chemists of old standing might find their advantage in a careful perusal of this section.

The remainder of the work consists of a graduated series of examples in simple gravimetric analysis; of a section on volumetric operations; of an account of the methods used in the valuation of ores, minerals, and industrial products; and lastly, of a special section on organic analysis.

Turning to the assay of pyrites, we find that the author directs the sulphur to be oxidised by means of chlorate of potassa, and not hydrochloric acid—as a certain standard work advises, with the ordinary result of a small explosion—but nitric acid, which works safely and well. The subsequent expulsion of the nitric acid, the rendering all silica insoluble, the removal of the last traces of sulphate of lead which may possibly be present, are all duly pointed out as needful. The addition of a little tartaric acid to prevent the precipitation of iron, along with the sulphate of baryta, is a sound precaution. We have seen precipitates of sulphate of baryta which, though thrown down from decidedly acid solutions, had a very tawny look. The digestion with acetate of ammonia to remove traces of nitrate of baryta which may remain entangled in the precipitate is also useful.

Turning to the separation of phosphoric acid from iron and alumina, we find that the author recommends the tin process (Reynoso's) to the exclusion both of the bismuth and of the molybdenum method, the latter of which we certainly prefer, whenever a comparatively small amount of phosphoric acid has to be determined in prescence of a large proportion of iron and alumina. The molybdenum process is, however, described and recommended for the determination of phosphorus in irons and

iron ores. It is remarkable how generally chemists overlook the fact that, when a substance has been ignited to remove organic matter, its phosphoric acid will be to a great extent converted into pyrophosphoric acid. Unless special precautions are taken for its re-conversion, the amount of phosphoric acid found will be decidedly erroneous.

The analysis of copper ores is very fully and clearly explained. Now methods so accurate, convenient, and rapid as the "Mansfield" and "Luckow's" are known, the question arises why the Cornish dry assay, which gives tolerably correct results only in the case of rich ores, should still be recognised?

Space will not permit us to examine this work at greater length. But although on certain points we should differ to some extent from the author, we can conscientiously recommend the work, not merely as a text-book for the student, but as a useful manual of reference to advanced and experienced chemists.

Workshop Appliances; including Descriptions of the Gauging and Measuring Instruments, the Hand-Cutting Tools, Lathes, Drilling, Planing, and other Machine Tools used by Engineers. By C. P. B. Shelley, C.E., Honorary Fellow of and Professor of Manufacturing Art and Machinery in King's College, London. London: Longmans, Green, and Co. 1873.

This also forms one of the series of Text-Books of Science, in course of publication by Messrs. Longmans, Green, and Co.; the subject embraced is, however, far too vast to be properly comprised within the pages of a single volume. As a text-book for novices, it may be of some value; and that part which relates to the use of hand tools will be found instructive, but it is the only portion of it which possesses any originality. The first chapter, "On Measures of Length, and Methods of Measuring," is almost a reprint from another source, but it is interesting, and forms a good introduction to the subjects subsequently treated of. The description of "wire gauges" is defective, inasmuch as no mention is made of Mr. Latimer Clark's proposed gauge, with which the author should be well acquainted. The latter half of the book, which relates to machine tools, is of but little use; the descriptive part is deficient, the mechanical details are insufficiently explained, and many of the illustrations are from old worn-out blocks which we recognise as having repeatedly seen in manufacturers' illustrated trade circulars. This portion might well have formed the subject-matter for a separate volume, and no doubt, if more space had been available for the purpose, Mr. Shelley would have done it fuller justice than he has been able to do within the limits to which he appears to have been confined.

Sanitary Engineering: a Guide to the Construction of Works of Sewerage and House Drainage; with Tables for Facilitating the Calculations of the Engineer. By BALDWIN LATHAM, C.E., M. Inst. C.E. London: E. and F. N. Spon. 1873.

MR. BALDWIN LATHAM is a well-known authority on all matters relating to Sanitary Engineering, and his experience in the construction of town sewers and the disposal of sewage has been such as to enable him to write from actual experience upon the subjects treated of in the volume now before us. Much of the information given in this book has appeared piecemeal elsewhere, having formed the subject of papers read before the Society of Engineers, and of pamphlets previously published by the author; but these are so scattered as not to be always available—or, indeed, convenient for reference—to anyone desirous of studying the important question of Town Sanitation. In "Sanitary Engineering," these previous writings by Mr. Latham have been collated and published, with the addition of a considerable amount of further information and experience, which together form one of the most valuable publications we have met with upon this important subject. In dwelling upon the importance and necessity of sanitary measures, the whole community is appealed to in a way easy to be understood by the non-scientific world, whilst in dealing with the manner in which such measures can best be carried out, Mr. Latham's experience cannot fail to prove of considerable value to future labourers in the same field of engineering science. At various times and places, earth, air, fire, and water have respectively been advocated as the best means of disposing of fœcal matter from towns, but, from a comparison of London with other great centres of population, where different methods have been applied, it is argued that water-carriage "is the best adapted to the varied requirements of a town population for effecting the speedy removal of the principal matter liable to decomposition."

In carrying into effect the works necessary for the sewerage of a town, three distinct operations have to be performed, viz.:—(1) The drainage of the surface; (2) The drainage of the subsoil; and (3) The removal of fœcal and other liquid refuse. In these several operations, the mode of dealing with the rainfall of districts comes under the first heading, and this is comprehensively dealt with, having due regard for varying situations, the geological character and the physical outline of different districts. On the subject of sewers—their form and relative capacity—the information afforded is most complete, accompanied as it is by tables of discharge, &c. The course and sectional form of sewers is well explained; but, perhaps, the most valuable portion of the book for engineers is that which relates to their construction under varying circumstances and in passing through different geological formations, not the least important part of the subject being that which relates to

the construction of sewers through sand and water-bearing strata. The flushing and ventilation of sewers occupies a considerable portion of the work, proportionate to its importance; whilst the concluding pages are devoted to the various forms of water-closets, &c., having special reference to the best means of connecting house-drainage with sewers. On the whole, this is a work of considerable merit, and the author has dealt with his subject in a clear and comprehensive manner, leaving little if anything to be desired.

Annual Record of Science and Industry for 1872. Edited by SPENCER F. BAIRD. New York: Harper, Bros. London: Sampson, Low, and Co.

THE second yearly volume of this work has just reached us. It reminds the reader, in some respects, of the well-known "Year-Book of Facts," but it takes a wider range, and enters more into detail. The "General Summary of Scientific and Industrial Progress during the Year," which is placed as an introduction, is carefully and fairly compiled, and gives a clear view of the course of scientific discovery during the past twelve months. The references to the various Transactions, journals, &c., quoted, are made on a novel and, we think, useful system. Each is designated by a letter and a number, the former signifying the country where it is published, and the latter the individual paper. An "Index to the References" gives the explanation. Thus, A 1 signifies the "Chemical News." The number of German scientific periodicals is significant, exceeding as it does that of the English and French taken together. The authorities quoted vary very much in value, and to some of the paragraphs the editor has prefixed a judicious "it is said." An extract from a German journal recommends the addition of *ammonia* to lessen the amount of sugar required in preserving acid fruits. An excess of ammonia, we are told, can be remedied by the introduction of a little vinegar. As the salts of ammonia. especially the acetate, have a decidedly nauseous flavour, we hope no lady will be persuaded to try this method.

Sanitary science is treated at great length. We notice paragraphs calling attention to the danger of using soaps made from putrescent animal matters, and extracts from Mr. Husson's paper on the milk of cows suffering from cattle-plague. A paragraph on the comparative value of antiseptics, taken from the "Academy," assigns to benzoic acid a higher power than to carbolic. "Metallic salts," such as sulphate of copper, occupy the highest place, whilst "inorganic salts," with the exception of bichromate of potash, "have but little power." These two statements seem scarcely reconcilable. General Scott's process for utilising, or, rather, for wasting sewage is described at length

in an extract from the "English Mechanic." We also meet with the "recommendation" of the Rivers' Pollution Commissioners. We trust that this is a "last appearance" prior to its departure for that "limbo large and broad," of which Milton sings. There are many interesting paragraphs in this book to which we may recur on a future opportunity. The work further contains a list of eminent scientific men who have died within the year, and concludes with an elaborate index.

The appearance of this volume is a proof of the increasing demand for scientific literature on the other side of the Atlantic. It is not improbable that in the course of a few years America may occupy as prominent a position in chemical science and in chemical manufactures as she has already done in the mechanical arts.

Report on Béton Aggloméré; or Coignet-Béton, and the Materials of which it is Made. By O. A. Gillmore, Major Corps of Engineers, Brevet Major-General, U.S.A. Washington: Government Printing Office. 1873.

Practical Treatise on Limes, Hydraulic Cements, and Mortars By Q. A. Gillmore, A.M., Brigadier-General of U.S. Volunteers. Fourth Edition, revised and Enlarged. New York: D. Van Nostrand. 1873.

"Beton Aggloméré" is a term presumably unknown to the major portion of mankind; "Béton," however, under its synonym of "concrete," is to us dwellers in towns a well-known material. Béton aggloméré is a concrete of superior quality, an artificial stone, in fact, prepared under certain conditions from given materials. The conditions are the use of the best materials; the use of only sufficient water to convert the matrix of lime or cement into a stiff, viscous paste; the incorporation of the solid ingredients, as sand, with the matrix by a thorough or prolonged mixing or trituration, producing an artificial stone paste, incoherent in character until compacted by pressure, by which every grain of sand and gravel is completely coated with a thin film of the paste; and, finally, this béton, or artificial stone, is formed by thoroughly ramming the stone paste, in thin, successive layers, with iron-shod rammers. The materials employed in making this béton are sand, common fat, or hydraulic lime, or Portland cement. Having given a concise statement of particulars and conditions favourable and unfavourable to the formation and induration of this new artificial stone, General Gillmore proceeds to consider, in a lengthy course of actual experiment, its merit and demerit. This consideration is accompanied by a detailed description of the mortar-mixing mills, the proportion of materials employed in, and the tensile strength and other properties of the result of, the process. To complete the means of comparison a series of experiments and abstracts

of experiments on Ransome's siliceous concrete, the Frear stone, the American building-block, the Sorel artificial stone, and Portland stone, are added. For the results, the reader—and even the general reader will derive much useful information from the work—be he builder, architect, or engineer, should refer to the actual description of the experiments. A sample of the work, however, may be epitomised from the chapter on artificial Portland cement, and its production by the English wet and the German dry processes. In the wet process we are told that the works in the vicinity of London employ both white and grey chalks of the neighbourhood, and clay procured from the shores of the Medway and the Thames. The clay and the chalk are mixed together in the proportion of about one to three by weight, and when a thorough mixture is effected in the wash-mill, the liquid, resembling whitewash in appearance, is conducted to large open reservoirs called bocks, where it is left to settle. When the raw cement mixture has attained the consistency of butter, it is shovelled out of the bocks, removed to stoves heated by flues, and dried. When dry, it is burnt with gas-coke in perpetual kilns. The cement-clinkers formed during the burning are ground into the powder known commercially as cement. By the dry process, the chalk, or marl and clay are kiln-dried, mixed in suitable proportions, and reduced to powder. This powder is moulded into bricks from a stiff paste; the bricks are dried, burnt, and ground, as in the wet process. These processes, described in four or five pages, including the proportions giving best results, are supplemented by remarks which will be useful to every practical engineer. General Gillmore's work next quoted in our list is equally full of information, but deals with the class of hydraulic limes and cements only thus admitting more general detail. Both works are likely to be of much practical utility to the builder or engineer.

Mind and Body: the Theories of their Relation. By ALEXANDER BAIN, LL.D., Professor of Logic in the University of Aberdeen. Second Edition. Henry S. King and Co. 1873.

PROFESSOR BAIN is well known as the author of a work on Logic, and of various papers in the "Fortnightly Review," one of which, "On the Theories of the Soul," is printed as the concluding chapter of the volume before us. He is also known as a prominent member of the Rationalistic school. We fear the fact that this volume has reached a second edition in a very short space of time is a sign that Rationalistic literature is eagerly read in this country, and that the attitude of mind which it develops is largely on the increase.

The subject which Professor Bain discusses in this volume is the precise connection between the mind and the great nervous

centre, the brain. He seeks to answer the question, "What has Mind to do with brain substance, white or grey? Can any facts or laws regarding the spirit of man be gained through a scrutiny of nerve fibres and nerve cells?" He reminds us, in the first place, of the very intimate connection existing between mind and body; the dependence of our frame of mind upon hunger, fatigue, bodily illness, stimulants, &c. "Bodily affliction," he remarks, "is often the cause of a total change in the moral nature." A little reflection will enable us to see that, although the mind is influenced more or less directly by other organs, the brain is the chief mental organ, and the mind is influenced through it. The brain is a very large and complicated organ; it is computed to receive no less than *one-fifth* of the entire amount of blood in the organism, which circulates through it, and this surely indicates some considerable activity. Physiologists have proved that the brain is indispensable to thought, feeling, and volition. Clever men usually have large brains, or brains with numerous and deep convolutions. Thus, while the average European brain weighs 49·5 ounces, that of Gauss weighed 52·6, De Morgan 52·75, Dr. Abercrombie 63, and Cuvier 64·5 ounces. We should like also to know the brain-weights of Descartes, Humboldt, Mozart, and Sir Walter Scott. Among idiots, brain-weights as low as 15, 13, and even 8·5 ounces have been found. An ordinary man, Professor Bain remarks, could not retain in his memory more than one-third or one-fourth, perhaps even less, of the facts remembered by Cuvier. "There would be no exaggeration in saying that while size of brain increases in arithmetical proportion, intellectual range increases in geometrical proportion."

An interesting account of the minute structure of the brain is given in the third chapter, from which we learn that the grey matter of the brain is composed of nerve-fibre mixed with small pear-shaped corpuscles, with prolongations to connect them with the nerves. The average diameter of the fibres is one six-thousandth of an inch, while the corpuscles range from one three-hundredth to one three-thousandth of an inch in diameter. The number of nerve-fibres constituting the optic nerve is very large, —probably there are as many as one hundred thousand. A few useful woodcuts accompany these descriptions.

In the next chapter we find, among other things, a physical theory of pleasure and pain, and in a long note (p. 75), we have the author's views regarding corporal and capital punishment. In place of hanging, he advocates an electric shock, and in place of flogging (which is revolting to the spectator, and inflicts permanent damage to the tissue), a succession of sufficiently severe magneto-electric shocks. The fifth chapter treats of the intellect, and at the outset destroys the long-existent idea that thought may be conducted in a region of pure spirit, unassociated with anything material. This is disproved by the fact that thought exhausts the nervous system, just as bodily exercise exhausts

the muscles. Have we not even heard of people thinking themselves hungry? The "physical seat of ideas" is discussed in this same chapter, where we also find a physical treatment of memory, retention, or acquisition, which is defined as "the power of continuing in the mind impressions that are no longer stimulated by the original agent, and of recalling them at after-times by purely mental forces." Professor Bain explains memory as an effect produced by the continuation in a weaker form of the original impression which evoked the original nerve-current. Just as when we hear the last clang of a bell an after-impression of a feeble kind remains on the ear. But surely we cannot imagine, in the case of memory, that the nerve-currents are always flowing; if so, why is the effort of memory ever necessary? If so, again, have we not motion produced from nothing, or at least an original impulse producing indefinitely continuous impulses, after the manner of perpetual motion?

The book is full of sound logic; it is the work of an accurate and active mind. It is a physico-metaphysical treatise, and to the man of science sadly lacks the absoluteness of the experimental fact. It is most pleasurable to read this book, yet we confess that when we arrive at the last page we find ourselves just as wise (or rather, ignorant) as we were before, in regard to the nature of mind.

On the Origin and Metamorphoses of Insects. By Sir JOHN LUBBOCK, M.P., F.R.S. Illustrated. Macmillan and Co. London; 1874. 108 pp., crown 8vo.

SIR JOHN LUBBOCK is well known to the world as an archæologist and anthropologist, and perhaps less well as an entomologist. Yet he has contributed no less than *thirty-five* papers to the Royal Society, and to various magazines, on entomology during the last twenty years; and, as he is not yet forty, we perceive that he must have studied the subject at a very early age. His first paper, "On Labidocera," appeared in the "Annals and Magazine of Natural History" for 1853.

The little work before us embodies in a popular form many of the more interesting results of his observations condensed from the above-mentioned memoirs. The articles have already appeared in "Nature," and the work forms the second volume of the *Nature Series* of books, which Messrs. Macmillan are now publishing.

The main subjects discussed are the classification, origin, and the nature of the different metamorphoses of insects; various views are traced, from the old standard "Entomology" of Kirby and Spence, one of the Bridgewater treatises, to the more recent memoirs of Müller, Agassiz, and Packard. The intelligence of insects comes out in a remarkable light. Many of our readers will remember Sir John's tame wasp at a recent meeting of the British Association. He remarks "we are accustomed to class

the anthropoid apes next to man in the scale of creation, but if we were to judge animals by their works, the chimpanzee and the gorilla must certainly give place to the bee and the ant." For example (p. 11), the larvæ of certain insects require animal food as soon as they are hatched, and the mother-insect consequently provides them with caterpillar and beetles, by burying them in a cell side by side with the unhatched larva. But here a difficulty arises: "if the Cerceris were to kill the beetle before placing it in the cell, it would decay, and the young larva, when hatched, would only find a mass of corruption. On the other hand, if the beetle were buried uninjured, in its struggles to escape it would be almost certain to destroy the egg." Look then at the wonderful, but diabolical, instinct of the creature. "The wasp has the instinct of stinging its prey in the centre of the nervous system, thus depriving it of motion, and let us hope of suffering, but not of life; consequently, when the young larva leaves the egg, it finds ready a sufficient store of wholesome food." A certain species of ants keeps *Aphides* in bondage, just as we do cows, for the sake of the honey-dew which they collect; a certain kind of red ant is indolent, and keeps black ants to do work for it. Once more, there is a kind of beetle which is blind and helpless usually found in ant's nests; the ants care for all their wants and nurse them tenderly. These things, and much more, of the lives of insects are told us in popular language in Sir John's book, which we recommend, not alone to the entomologist, but to the general reader.

Ozone and Antozone: their History and Nature. By C. B. Fox, M.D. London: J. and A. Churchill.

If, as Dr. Fox not unjustly remarks, "to the philosopher, the physician, the meteorologist, and the chemist there is perhaps no subject more attractive than that of ozone," it must be conceded that there are few subjects in experimental science more fraught with difficulties and involved in doubts. Since Schonbein first announced his discovery more than thirty years of research and observation have elapsed, yet the very existence of ozone is little more than generally conceded. As to the laws of its occurrence and distribution, its properties and functions, the means for its recognition, and its artificial production, there is a singular amount of discrepancy in the results of different observers.

As to antozone, it is still regarded by many chemists—in England at least—as little better than mythological. Such being the case, there is evidently room and need for a work like the present. Dr. Fox has undertaken the laborious task of giving a digest of the most important facts connected with ozone and antozone, comparing and, as far as practicable, harmonising, the views of former investigators. He endeavours to "point out the circumstances, and the manner in which, and the reason why, ozone is observed in the atmosphere," and finally gives the results of his own observations.

The concluding section, on the methods of observing and detecting ozone in the atmosphere in the air, is without doubt the most interesting and valuable. The author concludes that iodised litmus and simple iodide of potassium are the only tests that can be considered trustworthy. For the precautions to be observed in their employment we must refer to the book itself. We are bound to declare that, in our conviction, Dr. Fox has merited well at the hands of the scientific public for the elaborate and well-arranged digest of facts which he has placed at their disposal, and we join him in the hope that his labours will furnish a sound basis for future investigations.

Elementary Treatise on Physics, Experimental and Applied, for the Use of Colleges and Schools. Translated and Edited from Ganot's "Eléments de Physique." By E. ATKINSON, Ph.D., F.C.S., Professor of Experimental Science, Staff College, Sandhurst. Sixth Edition, revised and enlarged. London: Longmans and Co. 1873.

THE most prominent enlargements to this edition consist in further elucidation of the laws of the polarisation of light, a description of Gramme's continuous magneto-electric generator, and a further development of the theory of heat as recently advanced. We are glad also to notice an extension of fundamental formulæ. Nothing need be said in further praise of so eminently a standard work.

A Treatise of Medical Electricity, Theoretical and Practical; and its Use in the Treatment of Paralysis, Neuralgia, and other Diseases. By JULIUS ALTHAUS, M.D., M.R.C.P. Lond. Third Edition, enlarged and revised, with 147 illustrations. London: Longmans, Green, and Co. 1873.

IN religious dogma everyone knows the old saying that described orthodoxy as "my own peculiar doxy," and heterodoxy as "everybody else's doxy." But dogma (taken in its sense of enforced opinion) is not peculiar to religious discussion; the next elder science, that of medicine, has been full of it from the days of Æsculapius and Galen till now. Even now we are not free from the error of recording opinion instead of fact. And such a view is impressed upon all students of electro-medical science. The opinions held in contrast with the facts ascertained and stated without bias, are in overwhelming proportion. Indeed, the proportion is so great, that the influence of error is perceptible upon the minds of the public, who are too apt, perhaps, to regard electro-medical practitioners with not a little undue severity. It is then the duty of a scientific serial to uphold the careful chronicle of facts, and discourage the register of mere opinion, founded, if of any foundation, upon incomplete observation of fact. Thus it is our duty to commend to those who may be about to become interested in electro-medical science the

perusal of Dr. Althaus's book, and to express congratulation upon the attaining of a third edition. But not only will the reader in search of medical knowledge find here what he requires, but the general electrician will find also complete record of observations as to the action of electricity upon physiologically organic substances.

A Treatise on Acoustics in Connection with Ventilation, and an Account of the Modern and Ancient Methods of Heating and Ventilation. By ALEXANDER SAELTZER, Architect. New York: D. Van Nostrand.

MR. SAELTZER has given his subject great attention, and this attention has led him to some highly original views upon the relation of the propagation of sound to the density of the air, and the propagation of sound waves across strata of different density. Given the air and the propagation of sound waves across strata of different density, the author shows how it is that the voice of a speaker, whether proceeding from pulpit, platform, or proscenium, may be so reflected by the surface of the different air-strata as to be inaudible or confused to the hearers above or below the strata in which he speaks. Mr. Saeltzer next recounts the experiments that have led him to adopt certain principles of ventilation as aids in developing the acoustical properties of public buildings, by breaking up the air-strata. The book should be read, not only by the architect, but by every professional man whose duty it is to speak in public.

Experimental Research on the Causes and Nature of Catarrhus Æstivus (Hay-Fever, or Hay-Asthma). By C. H. BLACKLEY, M.R.C.S. Eng. London: Baillière, Tindal, and Cox, King William Street, Strand.

Mr. Blackley may be fairly congratulated on having produced a work which is not merely a valuable contribution to our medical literature, but which will be read with interest by many scientific men not connected with the profession.

Long-Span Bridges. By B. BAKER, Assoc. Inst. C.E. Revised Edition. London: E. and F. N. Spon. 1873.

To the student or to the engineer who desires to investigate the comparative theoretical and practical advantages of the various adopted or projected types of bridge construction, whether including box-plate, lattice, or bowstring girders, ribs, or suspension, we cordially commend this little work, as readable as it is accurate. The work really includes two sections, relating respectively to long- and to short-span bridges, and is illustrated with diagrams of type form built upon the long-span system.

PROGRESS IN SCIENCE.

MINING.

Statistics of considerable interest in connection with the coal-produce of this country have been published during the past quarter. The official returns furnished by the twelve Government Inspectors to the Home Office show that in 1872 there were 3016 collieries at work in Great Britain. In the previous year the number of mines was 3100. But in 1871 the number of coal-miners employed reached 418,088, whilst in 1871 the colliers numbered only 370,881. It appears that during 1872 the amount of coal raised was 123,393,853 tons, compared with 117,439,251 tons during the preceding year. This gives a decrease in the amount of coal raised per man; for whilst the average quantity gotten by each miner in 1872 was 295 tons, it amounted to 316 tons per man in 1871. As to the accidents, we note an increase in their number, but a slight decrease in the resulting deaths. Thus, in 1872 there occurred 894 separate fatal accidents, resulting in the loss of 1060 lives; in 1871 there were only 826 similar casualties, but they represented a sacrifice of 1075 lives. As usual only a small proportion of the deaths could be traced to explosions of fire-damp; indeed, out of the 1060 deaths in 1872 only 154 were due to such explosions.

It is not long since we called attention to some investigations on the connection between colliery explosions and the state of the weather, undertaken by Mr. R. H. Scott, F.R.S., of the Meteorological Office, and Mr. W. Galloway, who is now Assistant-Inspector of Mines in Scotland. These observers have since extended their researches, and have recently published an analysis of the catalogue of accidents which occurred during the year 1871. On the whole there were 207 explosions of fire-damp, of which 52 were attended with fatal consequences. It is believed that 113 of these explosions, or 55 per cent may be traced to the low state of the barometer: and that 39, or 19 per cent, are referable to a rise of temperature; whilst the remainder are unexplained by either of these meteorological causes. The paper contains some valuable instructions respecting the method of recording barometric and thermometric observations; these, of course, deserve careful study by the miner, especially now that the Coal Mines' Regulation Act of 1872 requires that "after dangerous gas has been found in any mine, a barometer and thermometer shall be placed above ground in a conspicuous position near the entrance to the mine." With regard to atmospheric pressure, the authors remark that if the barometer, after having remained nearly stationary for several days, descend half an inch or an inch during the next two or three days, the miner may expect to find fire-damp in greater quantity than usual in cavities in the roof and in the higher parts of the workings, and it may also appear where it had not been previously detected. If the temperature rise to 55° or upwards, the ventilating current is likely to be retarded. A sudden fall of the barometer—an inch in twenty-four hours or so—or a further fall after it has been unusually low for a day or two, points to the possible escape of gas, and calls for increased ventilating power, especially if the diminution of pressure be coupled with a rise of temperature.

We may observe that Mr. Galloway obtained the first of the prizes offered by Mr. Hermon, M.P., for Essays on the Prevention of Colliery Accidents. This Essay, which has recently been published in the "Mining Journal," is worthy of attentive study by those who have charge of our coal-mines.

Coal-cutting machinery received a fair share of attention in the Mechanical Section of the British Association at the late Meeting. Mr. Firth, of Leeds, well known for his zeal in promoting the use of such machines, read a paper "On the Introduction of Working Coal-Cutting Machinery in Mines." In this communication he described in detail his own form of coal-cutter, worked

by compressed air ; and during the meeting he gave many of the members an opportunity of seeing it at work in his pits at West Ardsley. Mr. Firth's machine has already been described in this Journal, and we have also noticed his efforts to promote the introduction of coal-getting machinery by offering some time ago a premium for the best form of machine.

At the same meeting, Dr. W. J. Clapp described the "Universal Coal-Cutting Machine," used at Nant-y-glo ; this machine is driven by compressed air, but has the action of a borer rather than that of a pick. Mr. J. Plant, of Leicester, read a paper "On the Burleigh Rock Drill," and two examples of the drill were actively at work in the yard of the Church Institute, close to the room occupied by the Mechanical Section.

A new form of safety-lamp has been described before the North of England Institute of Mining Engineers, by Mr. Emerson Bainbridge, of Sheffield. The light is protected by a cylinder of glass below and of brass above, so that wire-gauge is scarely used in its construction.

"The Application of Compressed Air to Underground Haulage" was the title of a paper lately read before the Dudley Institute of Mining Engineers, by Mr. A. J. Stevens, of Newport, Monmouthshire. After comparing the cost of using steam-power underground with that of applying the steam to the compression of air, he described a small and apparently simple winding-engine, which he has recently patented, with the view of dispensing with animal power for underground haulage.

Deposits of coal, almost unrivalled in magnitude, are described by Baron Von Richtofen as existing in certain provinces in China. Whilst in the maritime provinces the coal-measures have been in some places completely removed by denudation, and in others only imperfectly preserved, some of the inland provinces are singularly favoured, and exhibit fine exposures of their coal-bearing rocks. The province of Shansi contains at least 30,000 square miles of coal-producing ground, occupied partly by a fine bituminous coal and partly by anthracite. Some of the coal-seams are said to attain a thickness of 30 feet. In fact China offers to the miner coal-fields which are said to rival, if not to surpass, even those of the United States.

A description of the iron-ores of the Bidasoa, in the Pyrenees, by Dr. Röhrig and Mr. Haas, has appeared in a recent number of the "Chemical News." The mineral seems to be chiefly spathose ore, or carbonate of iron, superficially changed by atmospheric influences into a brown iron ore, or hydrous peroxide. From the published analyses the ores appear to contain more or less manganese ; and, as no phosphorus is recorded, they would probably produce a pig-iron remarkably well adapted for conversion into Bessemer steel. The ores occur in highly-inclined lodes, coursing for the most part through syenite, though some of the lodes are found in adjacent Silurian rocks and others in Triassic limestones.

In the Island of Anglesey a valuable deposit of copper-ore has long been worked at the Parys Mountain, but it is a disputed point whether this represents the only great treasury of mineral wealth in that island, or whether copper-lodes sufficiently rich to be worked are not widely distributed. It consequently becomes a matter of interest to notice any local vestiges of old copper workings in this island. The Hon. W. O. Stanley has communicated to the Royal Archæological Institute some interesting notes on this subject. It is hardly to be doubted that copper was exported from Anglesey prior to the landing of the Romans, and, indeed, the mineral wealth of Mona may have attracted the Romans thither. About the year 1640 a cake, or *massa*, of smelted copper was discovered, bearing the inscription "Socio Romæ." It is said to have been found at Caerhên, the ancient Conovium. In 1840 a second cake was discovered in Llangwyllog, and in 1869 three cakes were found at Castellor. Two years later three cakes were dug up at Bryndon, Amlwch, and passed into the hands of Mr. T. F. Evans, who has described them in a recent number of "Iron." One of the cakes is remarkable for being stamped, on two hunches, with the letters I V L S. They are curious relics, which may

have been smelted for use in making the bronze weapons and implements known to have been used at an early epoch of civilisation.

METALLURGY.

Some valuable researches on alloys, especially on those of copper and tin, have been conducted at the Paris Mint by M. Alfred Riche, who has recently published his results in the "Annales de Chimie et de Physique." The fusibility of the alloys was determined by means of Becquerel's thermo-electric pyrometer, with platinum and palladium wires, constructed by Ruhmkorff. The author finds that most alloys of copper and tin suffer liquation at the moment of solidification, and hence it is almost impossible to obtain their true melting-points. Exceptions, however, are furnished by those alloys whose atomic constitution corresponds to the formulæ $SnCu_3$ and $SnCu_4$; these alloys are not liquated, and the former of them possesses peculiar properties, differing in colour, for example, from all others of this class. Tempering increases the density of bronzes rich in tin, and annealing diminishes the density of tempered bronze. Whilst steel is hardened by being suddenly cooled, bronze is softened by this treatment. Yet the bronze is not sufficiently soft to be readily worked; and it has long been a vexed question how the Chinese manage to work their tam-tams and other instruments of bronze. Experiments made many years ago at the Ecole des Arts et Métiers, at Châlons, showed that such objects might, though with much trouble, be wrought in cold-tempered bronze; but the process was too difficult to be employed industrially. It was afterwards asserted by St. Julien that the Chinese worked their bronze at a red heat; yet it was difficult to understand this, since bronze is extremely brittle at high temperatures. M. Riche appears to have settled the difficulty by showing that although bronze cannot be readily worked either cold or at a bright red heat, yet it is easily manipulated at intermediate temperatures. Taking advantage of this fact, M. Riche and M. Champion have succeeded in imitating the Chinese tam-tams.

With respect to unalloyed copper, Riche finds that its density when alternately submitted to mechanical treatment, tempering and annealing, is variously affected according as the metal is protected from or exposed to access of air; in the former case the mechanical action increases, and in the latter case diminishes the density. The introduction of a small proportion of iron gives considerable tenacity and hardness to copper. The author has also studied a number of alloys of copper and zinc.

Within the last year or two considerable attention has been directed to the so-called "Phosphor-Bronze," an alloy of copper and tin, with more or less phosphorus, according to the purposes for which it is intended. Messrs. Montefiore-Levi and Künzel have brought this alloy to great perfection, and have applied it to a great variety of industrial uses, whilst in this country it has been prominently brought forward by the Phosphor-Bronze Company. When the proportion of phosphorus is large the alloy is extremely fluid, and therefore well adapted for castings; whilst its fine colour and close texture recommend it for decorative work. Certain forms of the bronze are characterised by great ductility and malleability; and may be readily rolled, drawn, or embossed; whilst other varieties are as hard as soft steel, and may be worked into tools and cutting instruments for use in gunpowder mills. It has been much recommended for ordnance and small arms; whilst the miner has used it for the wire ropes of his winding machinery, and the iron-smelter for the tuyeres of his blast-furnace. Numerous experiments on the mechanical properties of phosphor-bronze have been made at Berlin and Vienna, and by Mr. Kirkaldy in this country.

A detailed account of some experiments on Swedish steel, conducted by Mr. Kirkaldy, at his Testing Works at Southwark, has lately been published. The specimens tested were manufactured at the Fagersta Works by Mr. Christian Aspelin, and have been exhibited at Vienna. The enquiry was directed to the behaviour of the steel under the action of tensile, compressive,

transverse, twisting, and shearing stresses; in fact, to all such strains as actually occur in engineering works.

A new mode of tempering steel has been suggested by M. Caron. His plan is to plunge the red-hot metal into water heated to about 55° C., and he also proposes to restore "burnt iron" by a similar process.

The nature and uses of modern steel formed the text of Mr. Barlow's address as President of the Mechanical Section at Bradford; and although it naturally dealt chiefly with mechanical questions, it is yet deserving of the metallurgist's attention. To the same section Mr. Joseph Wilcock contributed a paper on the Bowling Iron Works at Bradford, in which he traced the history of iron-making in this district, and gave an excellent description of these extensive works. The Bowling and the Low Moor Iron Works, both celebrated for the manufacture of boiler-plates, were visited by many of the members of the British Association.

Sir Francis C. Knowles has recently laid before the Society of Arts a description of his method of refining and converting cast-iron into either malleable iron or steel. His object is to effect the conversion without the waste of heat and material which attends puddling and other converting processes now in use. Sir Francis employs as his source of heat the combustion of gases rich in carbonic oxide, and mixed in due proportion with atmospheric air heated to 500° C. Having melted the pig-iron in a cupola with coke or anthracite, he collects the gases, frees them if necessary from carbonic anhydride, enriches them by addition of free carbonic oxide, and is thus enabled to produce by their combustion a temperature of 2500° C. If higher heat and greater rapidity be required, he uses the cupola-gases for heating his retorts, and employs pure carbonic oxide specially generated, and thus obtains a temperature of 2979° C. In either case the mixture of carbonic oxide and air is blown into the molten metal, thus producing carbonic anhydride and nitrogen at a high temperature; but this heat may be readily utilised, whilst the products of combustion are passed through kilns or retorts, containing anthracite or coke, and the carbonic anhydride is thus reduced to carbonic oxide, which is again available for combustion. The finery or converter used in this process is peculiarly constructed, with the view of withstanding the high temperature to which it is subjected, and the interior is lined with a mixture of protoxide of iron, manganese, emery, bauxite, and caustic soda; a highly basic preparation is thus obtained for the lining, as it is considered desirable that the cinder should also be basic, not containing more than 30 per cent of silica. To eliminate the sulphur and phosphorus, caustic soda and rich oxide of iron or manganese are employed; and when superior iron and steel are to be prepared the use of nitrate of soda or permanganate of soda is recommended. It will thus be seen that, so far as the oxidising agents are concerned, the process is only a modification of the Heaton process.

MINERALOGY.

Two distinct minerals are known in jewellery as *Cat's-Eye;* the one being an opalescent quartz enclosing asbestiform fibres, which, lying in parallel directions, give the appearance of a fibrous structure to the quartz; whilst the other mineral, sometimes called for distinction sake, "oriental cat's-eye," is a fibrous chrysoberyl. Some very fine cat's-eyes have been received within the last year or two from the Cape of Good Hope. These stones are generally of a rich brown colour, but occasionally blue, red, and white. Some authorities have regarded the Cape cat's-eye as a form of crocidolite, whilst others have supposed that it is only a coloured fibrous quartz; but its true character appears to have been determined by Dr. Wibel, of Hamburg. After an attentive study of the chemical and microscopical properties of the mineral in question, he comes to the conclusion that it is not strictly fibrous quartz, but a pseudomorph of quartz after fibrous crocidolite. He shows that the so-called brown fibrous quartz, originally described by Klaproth, is merely a mixture of pure white quartz with goethite, or hydrous peroxide of iron;

whilst the blue variety is essentially a mixture of white quartz and crocidolite. The fibrous character is, in both cases, due to the replacement of fibrous crocidolite by quartz; the brown variety being the product of a perfect and slow alteration, whilst the blue is the result of an imperfect and rapid alteration.

Among the ejections from the eruption of Vesuvius in 1872, there occurred certain minute crystals, which were described by Scacchi as a new species under the name of *Microsommite*. This species has recently received attentive study by Vom Rath, who, in spite of the unusually small size of the crystals, has had the patience and acuteness to examine them crystallographically. About 1500 of these little crystals, weighing together only $\frac{1}{10}$th of a gramme, have passed through his hands. The crystals are colourless hexagonal prisms, striated vertically, and terminated by dull basal planes. The hardness is about equal to that of felspar; and the specific gravity is 2·6. An analysis of the very small quantity at Vom Rath's disposal yielded:—Silica, 33; alumina, 29; lime, 11·2; potash, 11·5; soda, 8·7; chlorine, 9·1; sulphuric acid, 1·7. The following expression is given as the probable formula:—

$$(\tfrac{2}{5}K_2O,\tfrac{3}{5}CaO),Al_2O_3,2SiO_2+NaCl+\tfrac{1}{13}CaO.SO_3.$$

The origin of microsommite may be traced to the reaction of steam, charged with chloride of sodium, on the volcanic minerals leucite and augite.

Under the name of *Horbachite* a new ore of nickel has been described by Dr. Knop, of Carlsruhe. In the neighbourhood of Horbach, near St. Blasien, in the Black Forest, there occurs a deposit of this ore, which has at different times invited exploration. The ore has been described as a nickeliferous magnetic pyrites, but analyses of the pure mineral show that it is really a distinct species. The mean of several analyses gives:—Sulphur, 45·87; iron, 41·96; nickel, 11·98. From this result we may deduce the formula $4Fe_2S_3+Ni_2S_3$. Knop calls attention to this association of sulphides as unique, no metallic sesqui-sulphides having previously been known to occur in a native state.

Some recent observations by Dr. Schrauf, of Vienna, show that *Brookite*, one of the three native forms of oxide of titanium, must be referred to the oblique or monoclinic system. The crystals affect, however, a decidedly prismatic habit; hence the reason why previous observers have placed this species in the prismatic system. Schrauf recognises three distinct types of crystal in brookite.

Prof. Rammelsberg has laid before the German Geological Society some papers on mineral arsenides and sulphides. In one of these he discusses the mutual relations and the chemical constitution of the compounds of arsenic and antimony which occur native. He is led to regard the metallic arsenides not as true chemical compounds, like the sulphides, but as isomorphous mixtures of metal and arsenic; whilst the sulpharsenides are mixtures of these with bisulphides. What he says with respect to the compounds of arsenic applies also to those of antimony.

Some beautiful crystals of *Torbernite*, or uranium-mica, have been recently raised in Cornwall, we believe from the neighbourhood of Redruth. They are notable for exhibiting an unusual development of the pyramid, so that the crystals are very different in general appearance from the common tabular forms.

A preliminary description of the meteoric stone which fell, some years ago, at the Barratta Station, near Deniliquin, in Australia, has recently been published by Mr. Archibald Liversidge, of the University of Sydney.

Nefediewite is a new Russian mineral, described by Pusirewsky. It seems to be an amorphous substance, resembling lithomarge.

ENGINEERING—CIVIL AND MECHANICAL.

Harbour Works.—On the 25th November last, Mr. L. F. Vernon Harcourt read a paper before the Institution of Civil Engineers on the construction and maintenance of Braye Bay Harbour, Alderney. This harbour was designed by the late Mr. James Walker, C.E., and the works in connection with its construction were commenced in 1847. The Admiralty intended, in the first

instance, to make only a small harbour, but subsequently gave directions for the enlargement of the scheme. In 1856, the design, then in course of construction, consisted of a harbour of 150 acres, with a depth of water of 3 fathoms and upwards, sheltered to the west and east by two breakwaters. The western breakwater, about 4700 feet in length, had been constructed, but the eastern breakwater was abandoned; and the harbour was consequently exposed to winds blowing from any quarter between N.N.E. and E.S.E. The western breakwater was exposed to the whole force of the Atlantic, and the effect of the fury of the storms was increased at Alderney by the rapidity of the tides near the island, occasioned by a peculiar confluence of currents in the bay of St. Malo. The breakwater was constructed on the "*pierres perdues*" system—a mound of rubble-stone being deposited in the line of the proposed work from hopper barges towed out by steam-tugs. As soon as the mound was sufficiently consolidated, it was surmounted by the superstructure, consisting of a sea wall and of a harbour wall 14 feet and 12 feet thick respectively, flooded at first at the level of low water, and built without mortar, the intermediate spaces being filled up with rubble, the batter of the sea wall being 9 inches and of the harbour wall 4 inches to 1 foot. To protect the lower or quay level, a promenade wall, 14 feet high, was built on the seaside, consisting of two masonry walls set in mortar, with filling between. In 1860, when the superstructure had been carried out 2700 feet from the shore, the design was somewhat modified. The breakwater was narrowed by reducing the width of the quay to 20 feet, the batter on the sea face was altered to 4 inches in a foot, solid masonry was substituted for the concreted hearting, and the foundations of the harbour wall were commenced at the same level as the sea wall. The head was built in 1864. The foundations were laid 24 feet below low water level, across the whole width of the breakwater. The first nine courses, each 3 feet thick, consisted of concrete blocks faced with granite headers; the upper portion was built of masonry in cement. The most exposed face stones were joggled and dowelled together, and several of the corner quoins were further secured by iron bars and diagonal straps. Two red leading lights on the shore mark the entrance to the harbour at night. The cost of the works of construction and maintenance to 1872 amounted to £1,274,200, of which £57,200 was expended in repairs.

Water Supply.—With a view to improving the water supply of Paris, the Montsouris Reservoir is now in course of construction to receive the waters of the Vanne. It occupies an area of 54,000 square metres, or 13½ English acres, and will contain 300,000 cubic metres, or 66,000,000 gallons of water, being the amount of three days of the normal flow of the canal which supplies it. The entire work is constructed of stone and cement, the exterior walls being strengthened by oblique arches having a thickness of 3 metres. The bottom is level except at the approaches to the wall, where is a series of sumpts of little depth, separated by partitions in order to form a number of arches which support the interior gallery. In front of each of the recesses thus formed are pillars, carried up to support the vaults of the arches which form the top of the building. The river Vanne is expected to afford daily a supply of about 100,000 cubic metres, or 22,000,000 gallons of excellent water. Hitherto the daily supply of water to Paris per head, as a mean, has been but 24 gallons; with the addition of the Vanne water, this will be raised to about 34 gallons.

Sewage.—The great development of the industries of the town of Rheims, and its consequent increase of population, having led to the necessity of providing facilities for the disposal of the sewage, a commission was appointed to investigate the subject, and it was decided, in 1870, to establish on a large scale a system of chemical purification, and a direct application of the sewage to agricultural purposes. Two processes have been tried—purification direct by chemical means, and irrigation. The Suvern agent, composed of chloride of magnesia, of lime, and of tar, was tried without effect, and the application of sulphate of alumina was equally unsuccessful. The latter process did, however, produce a certain effect, but an imperfect one, and that at an enormous cost. The most successful trial was that made with a mixture proposed by

MM. Houzeau and Devédeix, composed of a combination of lime and aluminous lignites, which latter are found in abundance in the vicinity of Rheims. MM. Houzeau and Devédeix's process was subjected to an extensive trial, and 2,500,000 cubic feet were thus treated in 1872. The cost for the purifying mixture per cubic foot was very trifling, and from the experience gained from the experiments, it was found that for treating the whole of the sewage waters of Rheims in this manner, the outlay would amount to £7000 a year, besides the interest on capital expended; and the operation would produce about 33,000 tons of manure of an inferior value. Although, then, this agent of MM. Houzeau and Devédeix purified the water well, but at a high cost, the process could be considered in certain special cases as a complement to irrigation; but taken alone, it was evidently too costly. Irrigation was next tried, and, in consequence of the results obtained, the Sanitary Commission has recommended the acquisition of about 450 acres of suitable land. The estimated cost of works, &c., is £40,000, and the annual cost of maintenance and working about £3800.

Boilers.—At the recent Vienna Exhibition, M. J. C. C. Meyn exhibited two of his patent high pressure boilers. This boiler consists externally of two cylinders, of which the upper is the smaller. The furnace is half internal and half external, and requires but little building. There is no bridge to the grate, which communicates directly through a short vertical flue with a central combustion-chamber. This chamber is traversed by 76 flattened vertical water-tubes, which form one of the principal features of the boiler. They are wrought-iron welded tubes, with horizontal channels across their sides. Through these tubes the water circulates from the lower part of the boiler, and between them the flame must pass. From the roof of the combustion-chamber a double ring of tubes leads up to the upper part of the lower shell, the upper cylindrical shell being of such a diameter that it stands inside these rings of tubes. This upper shell is enclosed in a smoke-box of the same diameter as the lower shell, and made of sheet iron. The ordinary water-level is two-thirds up the height of the upper tubes, and the upper shell, therefore, serves as a steam dome of large capacity, and has apparently been really effectual in preventing priming. The steam is led away from the top of the boiler through a pipe which forms a triple ring round it in the smoke-box before it is allowed to escape.

A paper on the "Strength of Boiler Shells" has recently appeared in the columns of the "Nautical Magazine," in which it is stated that the Board of Trade Surveyors have all along recognised as a fundamental principle of their practice the following opinion, expressed by Sir William Fairbairn, in 1854, viz.:—"Steam boilers of every description should be constructed of sufficient strength to resist eight times the working pressure, and no boiler should be worked, under any circumstances whatever, unless provided with at least two—I prefer three—sufficiently capacious safety-valves." It appears the "Galloway's rule" is most common amongst these surveyors, the *rationale* of which is that in the absence of tests witnessed by an officer of the Board of Trade, the strength of iron is assumed to be 48,000 lbs. per square inch in plates and rivets; this includes the effect of friction at the joints, and supposes the holes to be drilled, the strain to be applied lengthwise to the plate, and the rivets to be Lowmoor, or equal to that in quality. When the rivets are subjected to double shear, or where the strain is applied crosswise to the plate, only 43,000 lbs. is allowed as the strength per square inch of the rivet, or of the plate respectively. The section of the shell is taken as the length of the boiler by the thickness of the plate; but practically, the length of the section will be greater than the length of the boiler on account of the doubling of the plates at the circumferential seams. When the double rivetting is zigzag, as it should always be in boiler shells, the section is increased about 7 per cent by this extra material. To give effect to this, the 48,000 lbs. was increased about 7 per cent, or to 51,520 lbs., or 23 tons. A great deal of very valuable information and calculations are given in this paper, relative to the proper strength of boiler shells, but space will not admit of our following out the subject further at present.

Railways.—A new line of railway 15½ miles in length was opened early in September between Bristol and Redstock. It joins the Great Western Railway at Bristol, and is laid on the narrow gauge system. At present only a single line of rails has been laid, but the arches are wide enough for a double line if necessary at any future time.

The remaining portion of the Devon and Somerset Railway, between Wiveliscombe and Barnstaple, 36 miles in length, has recently been completed. Owing to the line running at right angles to the principal valleys and water-courses of the district, the works are in some places very heavy, involving, in addition to deep cuttings and high embankments, several tunnels passing through the ridges between the valleys, and some large river viaducts. One of these, crossing the valley of the river Tone, near Wiveliscombe, is 110 feet high, and has four spans, each 100 of feet. The construction adopted being lattice girders carried on stone abutments and piers. Another viaduct across the valley of the river Bray, in Castle Hill Park, is 100 feet high, and has six spans, each of 100 feet. The construction being similar to that adapted for the Tone valley viaduct; and there is also a large iron bridge over the river Exe, near Dulverton.

The list deposited this year at the Private Bill Office of projects for consideration during the coming session embraces 395 notices, of which, however, only 244 are accompanied by plans. Amongst these 121 relate to railway schemes, 7 to tramways, and 65 bills belong to the miscellaneous class, which includes docks, harbours, gas and water-works, reclamation schemes, street extensions, and local improvements. Amongst the railway projects are the following:—A proposition to complete the Inner Circle of the Metropolitan Railway by means of a line from Aldgate to the Metropolitan District Railway at Cannon Street. The construction of a Metropolitan Inner Circle Completion, and Eastern Extension Railway, by which it is proposed to form a line from the Metropolitan District Railway in Queen Victoria Street to the Metropolitan Railway at Aldgate, with extensions to Mile End and Bow, and junctions with the North London and East London Railways. It also includes a new street from King William Street to Fenchurch Street, and the widening of the latter street. Besides these there are the following schemes affecting the Metropolis, viz., the Metropolitan and South-Western Junction Railway Company's project for a line in Fulham between the authorised Hammersmith Extension Railway at North End-Road, and their own authorised line at a point near where it crosses the river; the Wandsworth, Fulham, and Metropolitan Railway scheme for a line from the Wandsworth Bridge to the authorised Metropolitan and South-Western Junction Railway at Fulham; the Ealing, Acton, and City Railway, which will unite the Hammersmith and City Railway with the Great Western and Brentford line; and the Acton and Hammersmith Railway to unite the North and South-Western Junction and Hammersmith Extension Railways. There are several projects for improved communication with the Crystal Palace; and most of the principal lines of railway propose improvements in the shape of short lines and junctions whereby increased facilities will be afforded to the travelling public.

GEOLOGY.

Physical Geology.—Baron Von Richthofen has recently described the extensive sheet of mud-like strata which extends over Northern China. It is called "Loess," from its resemblance to the river-loam of Germany, and has a thickness varying from 1000 to 2000 feet. There is not a trace of this formation in Southern China, and its occurrence in the northern parts produces quite a different class of physical scenery. The expanse of loess is cut up by vast chasms, a thousand feet in depth, along whose bottoms the streams flow. As regards its origin, after showing that it extended from the sea-level to an altitude of 12,000 feet, the Baron stated that it must have been formed where it is now seen, and by sub-aërial agencies. Of these, one of the chief had been the wind and the fine dust-storms, which often lasted for many days together. Rains also were great agents in the accumulation. He had

examined hundreds of sections of the loess, and had never found any fresh-water shells, but myriads of uninjured land shells.

Mr. W. T. Blanford has brought forward evidence of glacial action in Tropical India in early geological times. Some peculiar beds, possibly of carboniferous age, consisting of large boulders, embedded in a fine silt, were considered by him as evidence of glacial action; the deposition of the boulders being due to the agency of ice.

Professor Von Baer has recently written a memoir on the Caspian Sea, to which Dr. Carpenter has drawn attention. Instead of the Caspian being intensely salt, like the Dead Sea, its waters had only about one-third of the usual saltness of ordinary sea-water. This was due to the precipitation of the salt in the lateral lagoons, where great beds were forming. These salt pans drained off the saline substances, and thus left the water pure.

Professor Herschel and Mr. G. A. Lebour have made some experiments upon the conducting power for heat of certain rocks. Of all the rocks experimented upon, shale resisted heat the most, whilst the metamorphic rocks were the best conductors. The experiments showed that the rates of conductivity of heat through rock-masses depended on the structure of the latter. A thick bed of shale would arrest it. Hence, denudation of rocks, if carried on to any great degree, would alter the quantity of heat conducted from the interior of the earth to the surface. The experiments threw considerable light on underground temperatures.

The Rev. O. Fisher, in a note on the "Origin of the Estuary of the Fleet, in Dorsetshire," expresses his opinion that it is the eastern half of a submerged valley, similar to, though on a larger scale than, the one which now forms the Weymouth Backwater, its former western side having been encroached upon and destroyed by the waves of the Weat Bay.

Mr. G. H. Kinahan has lately described the Water Basin of Lough Derg, in Ireland, and discussed the method of its formation. He points out that the bays and all wide stretches across the basin conform with the strike of lines of breaks or displacements in the adjoining country, while the minor features of the coast lines are due to the weathering along minor breaks, joint systems, or lines of bedding. It seems that in this basin all the changes in the bearings of the different lines of deeps are connected with the strikes of the different faults in Slieve Bernagh and Slieve Aughta, and that the islands, rocks, and shallows are on the upthrow sides of these lines of faults.

It appears, from a despatch of the British Vice-Consul at Rhodes, that a volcanic outburst has taken place in the island of Nissiros, one of the Sporades, in which there existed a volcano supposed to be extinct. Shortly before the 10th June, 1873, new craters opened in this volcano, and from them ashes, stones, and lava were ejected; many fissures, from which hot water flowed, were produced in the mountain, and the island was daily shaken by violent earthquakes. From Rhodes, at a distance of about 50 miles, the smoke rising from the new craters could be seen.

Stratigraphical Geology.—Mr. W. Whitaker (assisted by J. B. Jordan) has prepared a large block-model of London and its neighbourhood on a scale of 6 inches to 1 mile, coloured to show the geology of the area, which includes Hampstead, Wimbledon, Great Ilford, and Shooter's Hill. All the superficial deposits are represented, and the subterranean geology to a depth of 1100 feet is shown on the sides of the model, which is divided into nine pieces. This model is placed in the museum at Jermyn Street.

Perhaps the Sub-Wealden Exploration is the most important topic in British geological circles. Commenced in 1872, the depth at present reached is a little over 300 feet. The boring commences about 230 feet down in the known Purbeck Beds, the thickness of which previously known in Sussex was somewhat over 300 feet; according to Mr. Topley, about 230 additional feet of strata have been made known by the boring, and in this series are some valuable beds of gypsum. Professor Phillips has communicated the latest intelligence in regard to the boring, stating that the lowest beds now reached are of marine

origin, that a specimen of *Lingula ovalis*, a shell of the Kimeridge clay, has been examined by him, so that the beds reached are already below the Purbeck. He remarks that the great upper clays of the oolites have been touched without encountering shore sands or shelly oolites,—no Portlandian rocks have appeared. Clay deposits may be found for a considerable depth; there may be no triassic beds, and he thinks that the palæozoic rocks may be reached at no enormous depth, and with no unusual difficulty.

Mr. Whitaker has recorded the discovery of Thanet Sand on the northern outcrop of the London Basin, near Sudbury, where it had not previously been observed.

Palæontology.—Dr. Von Mojsisovics has published the first part of his geological investigation of the neighbourhood of Hallstatt, in which he describes the remains of *Cephalopoda* obtained from the Zlambach and Hallstatt beds. He indicates repeatedly, especially in connection with the genera *Lytoceras*, *Pinacoceras*, *Sageceras*, and *Arcestes*, that he can by no means arrange the *Goniatites* as a distinct generic series in opposition to the *Ammonites*. The triassic representatives of the above-mentioned genera are most closely related in all essential peculiarities of organisation and habit to Goniatitic predecessors; the *Ammonites* from the Permian sand-stone of Artinsk, Waagen's Permian *Ammonite* from the salt range of the Punjab, and certain triassic forms partially bridge over the gap which still exists between the older *Goniatites* and the *Ammonites* of the Trias. By far the greater part of the genera of *Ammonites* occurring in the Alpine Trias have their roots in the Palæozoic *Goniatites;* and some of them may apparently even be traced back into the Upper Silurian formations. The greater part of these Palæozoic genera become extinct in the Upper Trias, when they attain the height of their development, but at the same time show signs of senile degradation, analogous to the phenomena observed during the gradual extinction of the later Ammonitic types in the Cretaceous period.

Professor W. H. Flower has described a new species of *Halitherium* from the Red Crag of Suffolk. It is of especial interest as furnishing the first recorded evidence of the existence in Britain of animals belonging to the order Sirenia. This new species presents many characters common to the Manati and the Dugong.

PHYSICS.

Microscopy.—A new freezing microtome, for facilitating the cutting of thin sections of soft tissues, has been contrived by Dr. William Rutherford, Professor of Physiology in King's College, London. The construction of the machine will be easily understood by reference to the cuts. Fig. 2: A, hole in the brass plate (B); C, tube; D, screw; E, indicator; F, screw for fixing the machine to a table; G, box for holding the freezing mixture; H, tube for permitting the water to flow from the melting ice. Fig. 3: Vertical section. The hole A is shown containing a piece of tissue, and the box G containing the freezing mixture; K, a movable bottom to the hole A; R, transverse section of the knife used in making the sections: the other letters the same as in Fig. 2. The tissue to be frozen and cut is placed in the tube (A). The section is made, as in the ordinary instrument for cutting wood sections, by gliding a knife horizontally through the tissue projecting above the level of the plate (B). The thickness of the section is regulated by the indicator (E). The machine may be employed for two objects:—For cutting tissues hardened in the ordinary way, by chromic acid, &c.; and second, for cutting tissues hardened by freezing. The second method of using the machine will be more readily comprehended after a description of the first, which is simply this:—Place a portion of hardened tissue in the hole A, and pour around it a mixture of paraffin (5 parts) and hog's lard (1 part), melted by the aid of a gentle heat. Or the paraffin mixture may be first poured into the hole, and the piece of tissue afterwards introduced, and held in any desired position, by means of forceps, until the paraffin becomes sufficiently hard. In order that the paraffin may fairly support the tissue, it is necessary that the surface of the latter be

dry. This is easily accomplished by leaving it exposed to the air for some time, either with or without previous immersion in spirit. When the machine is used for the second object—that is, for freezing—the following directions are to be attended to:—Surround the freezing-box with two or three layers of flannel, and screw the machine to a table. Unscrew the movable bottom, or

FIG. 2.

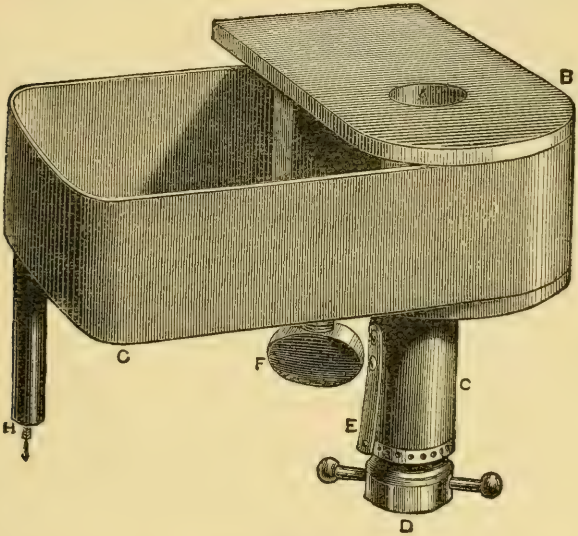
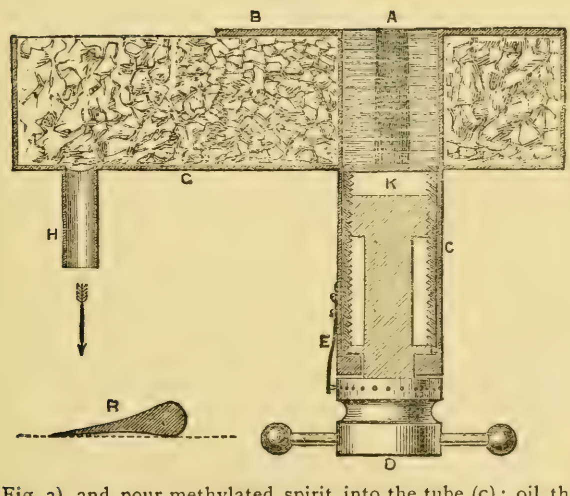


FIG. 3.



plug (K, Fig. 3), and pour methylated spirit into the tube (C); oil the side of the plug; replace it, and screw it down to any desirable extent, and there leave it. The object of this is to prevent the screw from becoming fixed by the freezing. The spirit which has come above the plug (K) must be thoroughly

removed by means of a towel, and the small slit at the margin of the plug carefully closed by means of hog's lard, which should be spread in a thin layer around the entire margin of the plug, to prevent the spirit from in any way reaching the cavity above the plug. The screw (D) must not be touched until after the freezing is completed, in case this accident occur. The tissue to be frozen, together with an imbedding fluid, are placed in the hole. For ordinary purposes a solution of gum arabic may be employed, prepared in the following manner:—Add to 10 ounces of water, 2 drachms of camphorated spirit and 5 ounces of picked gum arabic; when the gum has dissolved, strain the fluid through calico, and preserve for use in a corked bottle. The gum when frozen can be sliced as easily as a piece of cheese. The gum or other fluid should be first placed in the hole of the machine, and when a film of ice has formed at the periphery the tissue should be introduced and held against the advancing ice until it becomes partially frozen. In this way a portion of tissue may be secured, in any position, for the process of section. Lay a piece of gutta-percha upon the brass plate (B) so as to cover the cavity containing the tissues and prevent the entrance of heat, and the accidental entrance of salt from the freezing mixture. Place in the freezing-box (G) alternate quantities of finely powdered ice and of salt, and take care they are pushed round the tube of the machine, and also that the tube (H) is kept open, in order to permit of the constant egress of the water from the melting ice. The freezing can be most rapidly effected by the addition, at short intervals, of ice and salt, and by repeatedly stirring the mixture, in order that the escape of water may be facilitated. It is possible, especially in winter, to have the tissue frozen too hard to permit of its being readily cut. It splinters when it is too hard. This is prevented by discontinuing the further addition of the freezing mixture, or by dropping water or a three-quarter per cent salt solution, at the ordinary temperature, on the surface of the frozen tissue, or by heating the razor slightly. With regard to the cutting tool, it may be stated that a razor answers perfectly well for all ordinary purposes. The blade should always be *hollow* on both surfaces (R, Fig. 3). It is a mistake to employ a flat knife, for it is scarcely possible to keep the surface of the brass table of the machine smooth enough to permit of the knife lying quite flat. The knife should be *pushed* obliquely through the tissue, which should be cut at one sweep. This is not possible with a frozen tissue, if the ice be too hard. For an unfrozen tissue embedded in the paraffin the knife should be wetted with methylated spirit. In the case of freezing it is not necessary to wet the knife, for the melting ice does so readily. The machine has been made by Mr. Baker, of High Holborn.

Mr. John Barrow* recommends naphthalin as a material for embedding soft tissues for the purpose of cutting sections. The advantages claimed for naphthalin over wax and other substances are—A low fusing-point, absence of contraction, the minimum of injury to the edge of the knife, and very ready solubility after cutting in benzol or spirit, so that the substance is at once removed from the section without injury.

Mr. F. H. Wenham, at the October meeting of the Royal Microscopical Society, exhibited, under the microscope, a roughed pattern on a piece of glass produced by the American "sand-blast," remarking that the microscopical appearance of the "greyed" surface is quite distinct from that of ordinary ground glass. Observations under a low power give some insight into the *modus operandi*. It was stated, at the late meeting of the British Association, in the discussion that followed the description of the process, that a large crystal of corundum was speedily perforated with ordinary sea-sand and a blast-pressure of 300 lbs. per square inch. Corundum is several degrees beyond emery in hardness, approaching near to that of the diamond. But it was further stated that, under the conditions named, diamond itself became speedily worn away. At first sight it seems extraordinary that the hardest known material should quickly be destroyed by one considerably softer. The

* Manchester Literary and Philosophical Society, Microscopical and Natural History Section, April 21, 1873.

microscope indicates that this is caused by the force and velocity of impact; it is not a grinding process at all, but a battering action, similar to that of leaden bullets against a block of granite. A polished glass surface exposed for an instant to the sand-blast shows an aggregation of points of impact, from which scales of fractured glass have broken away in an irregular radial direction. It appears as if a pellet of glass had been driven in by the collision of the sand, and the wedge-like action thus set up had driven away the surrounding glass. All these spots or indentations, when tested by the polariscope, show a coloured halo round each, proving that the glass surface is under strain and ready to yield to further fracture. The action therefore is not so much due to the hardness of the striking particles as to the force and velocity of impact. This is sufficiently great to destroy the cohesion of the surface of the material operated upon. The external layer is carried against the under stratum, and the material is crushed and disintegrated by a portion of its own body.

Mr. William Webb, whose minute writings on glass are so well known to microscopists, disputed the reality of the finer bands of lines on Nobert's test-plate, on the ground of the tool cutting away the surface of the glass, and leaving, as he showed by certain diagramatic sections, irregular jagged furrows. Dr. Woodward, of the Army Medical Museum, Washington, in reply, reminds the readers of Mr. Webb's paper, that there is a physical reason which compels us to believe that the first fifteen bands, at least, of the nineteen-band plate are composed of real and distinct lines, and that the distance apart of these lines must approximate very closely to what was intended by Nobert. When the bands of Nobert's plate are illuminated by oblique light, and are looked upon from above with a low power (too low to show any of the lines), each band appears as a smooth coloured stripe. From the known wave-length of the colour seen, and the angle of the incident pencil, the distance which the lines of any band must actually be apart can be computed by the well-known formula for the spectrum of gratings enunciated by Frauenhofer, and the distance thus obtained agrees with that at which Nobert ruled the lines. On the other hand, the angle of the incident pencil being known, and Nobert's given distance being assumed to be true, a table of wave-lengths for the different colours may be calculated, and the wave-lengths thus deduced agree substantially with those computed by other means. Nobert has discussed the whole matter in the 58th volume of "Poggendorff's Annalen" (1852). His discussion leaves, in the opinion of Dr. Woodward, no room for the possibility of a doubt of the objective reality of the lines up to the fifteenth band. Dr. Woodward calls attention to the fact that this reason is altogether independent of our ability to resolve the lines with the microscope. In fact, it enabled Nobert to know that his plates were correctly ruled long before the resolution of any but the coarsest bands had been effected. As no spectral colour is obtained in the bands finer than the fifteenth, the formula of Frauenhofer cannot be applied to them. In fact, the formula demonstrates that if these bands are actually ruled, as claimed, they can give no spectral colour. Dr. Woodward has no hesitation in expressing the opinion that the four higher bands (sixteenth, seventeenth, eighteenth, and nineteenth) have also an objective reality. He bases this opinion upon the comparison of their optical appearances, as seen with the best glasses, with the appearances of the lower bands (especially those from the ninth to the fifteenth). These appearances are quite the same in both cases, and, as similar results follow similar causes, he infers the existence of real lines in the four higher bands, since he knows they exist in the others. Dr. Woodward has recently examined two new test-plates by Nobert,—the first ruled for Professor Barnard, of Columbia College, the second for the Army Medical Museum,—in which the maker has attempted to rule lines twice as fine as those of the nineteenth band. These plates have twenty bands. The first ten correspond respectively to the first, third, fifth, seventh, ninth, eleventh, thirteenth, fifteenth, seventeenth, and nineteenth of the old plate. The lines in the second group of ten bands purport to be ruled at the following distances apart:—The eleventh band, $\frac{1}{11000}$th of a Paris line; the twelfth band, $\frac{1}{12000}$th; and so on up to the

twentieth band, lines of which are said to be $\frac{1}{20000}$th of a Paris line apart. These new bands have not at present been resolved.

Mr. J. J. Monteiro, in a letter to the "Chemical News,"* gives some account of the habits of the "plantain eaters" (*Turacus albo-cristatus*), the birds from the feathers of which the colouring matter turacin was extracted by Professor Church, and which is distinguished by its remarkable two-banded spectrum. These birds are common on the West Coast of Africa, and in the part known to the writer, viz., from Loango, in 5° S. lat., to Fish Bay, in 15° S. lat., they abound. Over the whole country mentioned, and for a considerable distance inland, copper is found, most abundantly distributed, as malachite or green carbonate; specks of the green mineral are everywhere to be found. Mr. Monteiro considers it highly probable that the birds swallow small particles of the cupreous substance with the gravel, &c., so commonly taken by all birds. This may account for the large quantity of copper discovered by Professor Church in the feathers of these birds. Mr. Sidney Lupton also remarks† that two green love birds (*Melopsittacus undulatus*) were in the habit of pecking at the bars of their cage or any brass-work accessible to them. The feathers of these birds also yielded traces of copper. The spectrum of the green feathers had not been examined. The lower band of the turacin spectrum (Fig. 5) corresponds in position, although not in extent, with the band of ruby glass (Fig. 4): extent of absorption, however, is in many cases de-

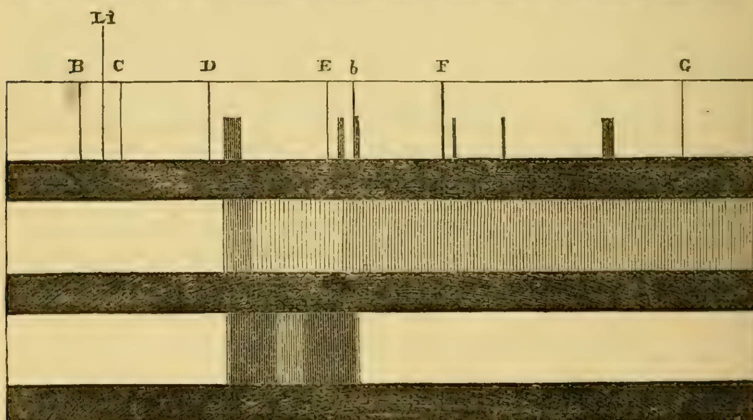


FIG. 4.
FIG. 5.

pendent upon the intensity of the coloured medium, many bands, as those of the aniline dyes, being easily made to absorb to a greater or less extent, according to the amount of dilution or the thickness of coloured medium through which the light is allowed to pass. (The bands between the Frauenhofer lines, extending half-way across the upper spectrum, are those of nitrate of didymium, and, from their sharpness and convenient distribution, are convenient in mapping spectra by artificial light).

ELECTRICITY.—Mr. Casselberry, of St. Louis, has been experimenting for several years with voltameters. He finds as the result that two voltametric apparatus (of the ordinary kind) connected together, not in series, but with their oxygen and hydrogen electrodes attached by wires, the oxygen electrodes to the positive, and the hydrogen electrodes to the negative pole of a battery, that there is then generated in *each* voltameter an equal quantity of gas to that generated in only one voltameter with the same battery. The experiment is borne out by the law of derived circuits, the second voltameter being

* "Chemical News," vol. xxviii., p. 201.
† *Ibid*, vol. xxviii., p. 212.

in effect a derived electrolytic circuit to the first. Should the experiments (and they are most carefully recorded) stand the test of general experience, the sequence of great importance in the practical deposition of metals would appear to follow naturally and immediately.

The experiments were repeated with a magneto-electric machine, with results precisely similar to those obtained with the battery.

The electric current has again been utilised for the purposes of ascertaining longitude, this time of Harvard observatory. The results show the difference of time between Harvard and Greenwich to be for the following years:—

	Hrs.	Mins.	Secs.
1867	4	44	31·00
1870	4	44	31·05
1872	4	44	30·99

The mean of which is 4 hrs. 44 mins. 31·01 secs. The close agreement of these results indicates a very near approximation to the truth, and this fundamental longitude may now be considered as settled. Adding to the above value the difference in time between the Washington and Harvard observatories, 23 mins. 12·12 secs., we get for Washington 5 hrs. 8 mins. 12·12 secs.

In America interesting lectures upon the system of fire, burglar, and other alarms as laid down by the American District Telegraph have been delivered. Particulars will be found in the "Journal of the Franklin Institute."

In natural science electricity has again made progress. Dr. Burdon-Sanderson has investigated the electrical phenomena which accompany the contractions of the leaf of *Dionæa muscipula*, and he has demonstrated their collateral character with those of nerve and muscle.

Investigation of the Fluorescent and Absorption Spectra of the Uranium Salts.—President Morton and Dr. Bolton, after briefly noticing the labours of their predecessors in this department of research, describe the methods employed, which do not differ from those of Stokes and Becquerel, except in small details, and the means at their disposal for securing accuracy in the measurements. A variety of uranium compounds were specially prepared, and their spectra, both of fluorescence and absorption, were carefully mapped out and measured. Illustrations are given showing some of the most characteristic of these spectra. Very characteristic differences were found between the spectra of certain salts, and in a number of cases one body can be readily discriminated from another by this means. Indeed, in this investigation impurities in many commercial compounds of uranium were thus detected, and, in other cases, the progress and consummation of a change in composition, or in the formation of a compound, was watched and recognised with the greatest ease and precision. In almost every case there is a tendency of the light to fade off in the bands towards one side more gradually than towards the other. In nearly all spectra this graduation is greatest towards the less refrangible end of the spectrum. The character of any one band is, as a rule, a type of all the bands of a spectrum; but to this a remarkable exception is found in the double acetates of uranium generally, and especially in the sodium salt whose fluorescence is the brightest. In the spectrum of this salt the first four bands at the lower end of the spectrum in the orange and red differ entirely in character and spacing from the rest, except the fifth, which seems to be in a transition state. It is also true, as a rule, that double salts with the same acid have bands of a like character; but to this also there are decided exceptions, and it is by no means true that all salts with the same acid have like bands. This chances to be true in the case of the sulphate and normal double sulphates, but in the case of the acetate and double acetates, fluoride and double fluorides, chloride and double chlorides, as also among the numerous hydrates of the sulphates, it fails to maintain itself. Nothing could well be more unlike than the spectra of uranic oxychloride and the potassium chloride. The question arises, how far the spectra of substances are constant, and in what way a change of spectra is to be interpreted. The authors find that no substance has its spectrum changed by anything which does not affect its composition, excluding the effect of heat, and certain cases

in which a substance has been caused to give a continuous spectrum in place of its normal one. We may, therefore, ascertain in many cases whether under certain treatment a body has or has not suffered a change in composition, and trace such a change step by step.

Absorption Spectra.—There are in the uranium salts two sorts of absorption —one directly related to their fluorescence, and the consequence of the fact that those rays which excite fluorescence must themselves disappear, their motion taking that other form; and the other an absorption having no immediate relation to fluorescence, but representing rays of the spectrum whose motions are converted into heat or some other form of force not sensible to the eye (*Phil. Trans.*, 1852, p. 520). Absorptions of the first class are best studied by direct observation, combined with a process closely allied to that described by Stokes as his *third method*, which consists in throwing a pure spectrum upon a screen of the substance in question, or upon the vertical side of a tank containing a solution. With the solid screen, the location of general maxima of fluorescence will correspond with maxima of absorption, and with the tank the absorption can be directly seen as embodied in dark blades or triangular masses of shade running into the tank (as seen from above) from the side away from the light. These appearances will often indicate the existence and relative intensity of absorptions, whose exact locations we can measure by examining the transmitted light directly with the spectroscope. The spectra of absorptions not directly related to fluorescence are, as a rule, best studied by transmitted light. The difference between different salts as regards their apsorption-bands is very great; and, while in many cases solution has a vast effect upon fluorescence, it sometimes produces but little effect upon the absorption-bands. In other instances, however, very marked changes occur, and, when these are followed out to their legitimate conclusions, they lead to some very remarkable results. Thus, if we examine the absorption-spectra of the uranic acetate and the various double acetates, we shall find that in the solid state they present great variety in the exact location of the bands, but in solution we have exactly the same spectrum for all. The conclusion therefore presents itself, that in solution all are reduced to the same state, which could, of course, only be by the breaking up of all the double salts. Indeed, from this, supported as it is by other observations, we do not hesitate to conclude that *no double acetate can exist in solution in water*, but must break up into its two single salts. Nor do our conclusions stop here, but we must reserve others until some of the facts on which they are founded have been described. A similar experience leads us to a like conclusion in the case of the sulphates, oxychlorides, &c. Attention was drawn to the fact of such displacement by one of us in September of last year, but its true bearing has only been perceived recently, since a large number of observations have been accumulated. A change of character, rather than of position, produced by solution in the absorption-spectrum of didymium sulphate, was observed by Bunsen in 1866 (see "Pogg. Ann.," vol. cxxxviii., p. 100, and "Phil. Mag.," 4th series, vol. xxxii., p. 181). The position of the band of uranium nitrate, while unaffected by solution in water, is notably changed by other solvents, as the following table will show:—

Bands.	1.	2.	3.	4.	5.	6.
Glycerin	87·6	96·0	107·8	123·0	136·0	148·6
Water	89·8	98·5	108·5	118·7	129·5	142·0
Alcohol	—	99·4	111·7	—	—	—
Hydrochloric ether..	—	99·4	110·8	123·7	—	—
Ether	—	100·0	112·6	123·0	—	—
Acetic ether	—	101·0	111·7	128·0	135·4	—

Effects of Heat.—It was observed by Stokes that canary glass and the nitrate of uranium had their fluorescence reduced by heating, and that at a temperature much below redness their influence upon light in this respect was quite suspended. In the solutions of nitrate of uranium all fluorescence was extinguished near 212° F. The substance regained its fluorescence on cooling. He also remarked that no such action appeared in fluorescent vegetable solu-

tions. Gladstone finds, as a rule, the effect of heat is equivalent to a concentration of the solution, and amounts to an increase in the amount of absorption. In a paper "On the Change of Colour produced by Heat in Certain Chemical Compounds," E. J. Houston pointed out the curious and novel fact, that, in all cases where no chemical change was involved, solutions, as well as solids, changed to tints lower in the spectrum on the application of heat. The loss of fluorescence in a few substances when heated appears to extend (with certain limitations) to all the uranium compounds, both in their solid state and in solution. We find that in the case of the anhydrous ammonio-uranic sulphate, fluorescence is sensibly diminished at 140° C., and is almost destroyed at 260° C. The hydrate does not show any marked loss of fluorescence below the point at which it begins to part with its water. The same is true of the potassium sulphate. The sodio-uranic acetate is much more sensitive. Experiments were made with it and other salts. It was observed that at about 50° C. the brightness of the fluorescence was reduced, and that the uppermost decided band (81·8) lost its distinctness. At about 116° C. it seemed to reach a minimum. At that temperature the uppermost band had vanished, and the lower ones were too faint for measurement. No further change was noticed on carrying the temperature to 150°. Solutions are still more sensitive. The authors next enter upon a detailed examination of various uranium compounds, from which we select the following passages:—

Uranic Acetate (*normal*), $U_2O_3C_4H_3O_3+2HO$.—This substance fluoresces very brightly, different specimens, however, differing, probably from the pre-

FIG. 7.

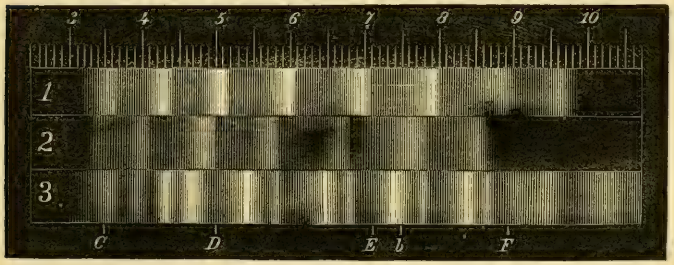
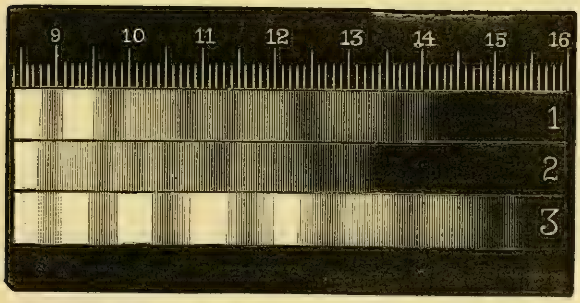


FIG. 8.



sence of minute traces of foreign matter. Its solution yields a very bright fluorescence, which is reduced by the addition of a trace of alcohol, ether, glucose, or sucrose, and is destroyed by a very small amount of hydrochloric acid. The fluorescent light of the solution yields a continuous spectrum.

When examined with the spectroscope its fluorescent light emitted by the solid yields a system of eight bands. Of these the 1st and 7th are very

FIG. 9.

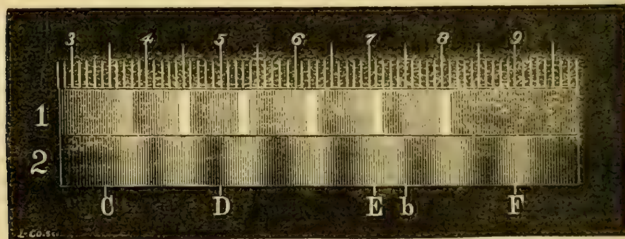


FIG. 10.

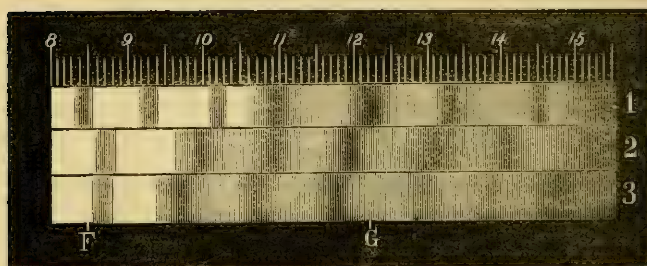


FIG. 11.

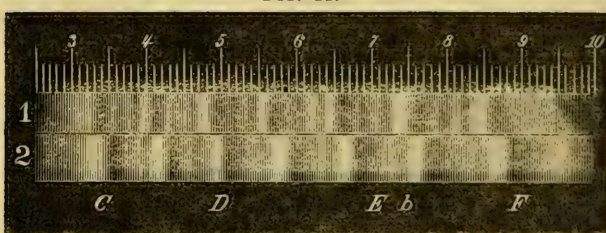
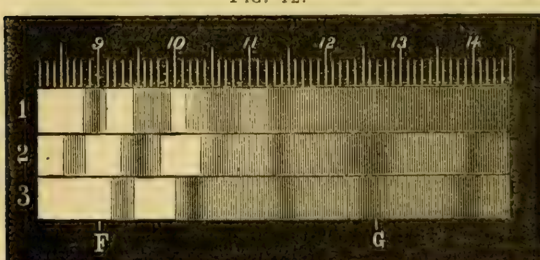


FIG. 12.



faint, and the 8th only appreciable with a strong light and wide opening to the slit of the spectroscope. The brighter bands show a very abrupt termination

towards the more refrangible end, and shade off gradually so as to look like pieces of moulding illuminated from the violet side of the spectrum. No. 1 in Fig. 7 will give an idea of this, as well as of the positions of the various bands. By turning the spectroscope obliquely on the bottle containing this salt, an absorption-band at 107 can be distinguished with ease, but none above this can be made out, and, as regards this matter, the strongest contrast exists between the uranic acetates and its double salts. By crushing a few grains to a fine powder, with a little water, between slips of glass, we may observe the absorption-bands by transmitted light with facility, and get a spectrum of a curious character as regards the irregular spacing of its bands. No. 1 of Fig. 8 will give a good idea of this. Another noteworthy point is the very strong general absorption, which almost obliterates details of the spectrum, and makes it impossible to recognise any bands above one at about 135. In solution this general absorption is increased, and the absorption-bands are blended so that little can be done in the way of measuring them. If we make a solution of the neutral acetate in water, and examine its absorption, we shall find a faint band at about 105, and some indication of one at about 117, but a very heavy general absorption over the entire region above the first-named band obliterating all variations of shade. The addition of a little acetic or other strong acid will, however (while destroying the fluorescence of the solution), clear up its absorption-spectrum in a remarkable manner, giving us such a one as is shown at No. 3 of Fig. 8, which we have reasons for regarding as the absorption-spectrum of *the double acetate of uranium and water.*

Anhydrous Uranic Acetate, $U_2O_3C_4H_3O_3$.—If this normal uranic acetate is dried, at a temperature of 100° C., for some hours, it becomes opaque, and of a lighter and purer yellow tint, and is found to yield a fluorescent-spectrum, the bands of which are like those of the normal salt, but are all displaced downward in the spectrum. No. 2 of Fig. 7 will show the arrangement of these bands. The brightness of the fluorescence is very much reduced, and the first and last bands could not be made out with the apparatus at present in use. By inclosing this substance in dry powder, between slips of glass, its absorption-spectrum was observed with transmitted light, and is shown at No. 2 of Fig. 8. The general absorption is greater in this than in the case of the normal uranic acetate.

The Double Acetates.—These bodies give spectra which, whilst differing remarkably from that of the single acetate of uranium, agree strikingly among themselves. The fluorescent spectrum of the sodio-acetate of uranium is a type of a perfect double acetate of spectrum. All the double acetates show their absorption spectra with very great ease. The arseniates appear peculiarly fixed and inflexible in the relations under review. The four compounds examined exhibit but one spectrum of fluorescence and one of absorption. The characteristics of the former are seen in Fig. 9, No. 1, and those of the latter in Fig. 10, No. 1. The double carbonates of uranium fluoresce faintly, showing a character like that of the less brilliant double acetates. See

Fig. 13.

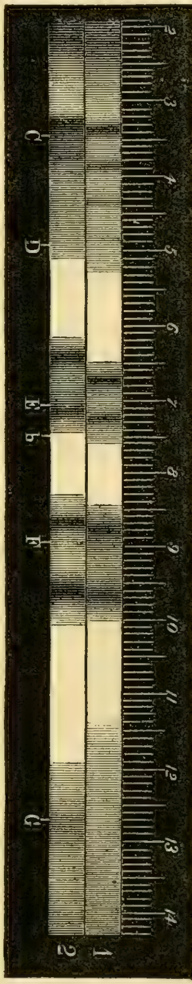
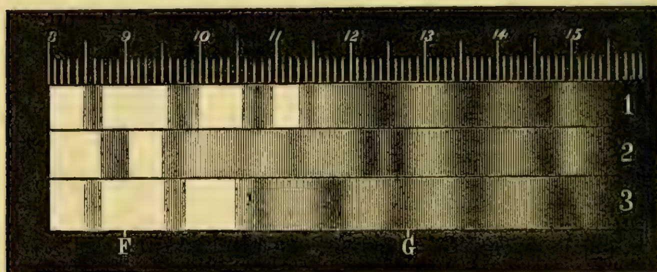


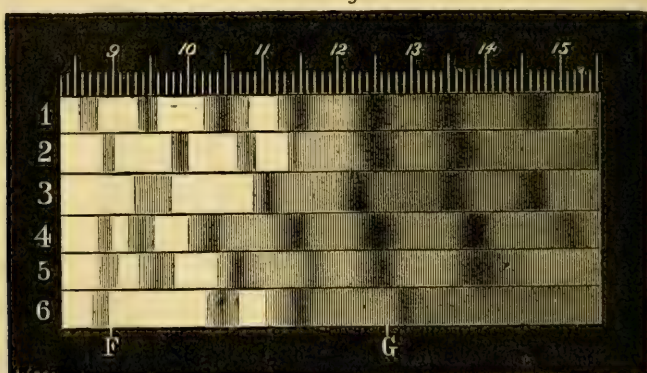
Fig. 9, No. 2. The neutral oxychloride has a very great general absorption, and shows some bands, but is almost devoid of fluorescence. The uranic oxyfluoride gives a spectrum generally resembling the acetate, normal sulphate, &c., and shown in Fig. 11, No. 2. The absorption-spectrum is likewise well marked, and is shown in Fig. 12, No. 2. When dissolved in water and acidulated with hydrofluoric acid, it yields the absorption-spectrum No. 3 of Fig. 12. Of the double oxyfluorides the potassium salt is the most brilliant, and is represented in Fig. 11, No. 1, its absorption-spectrum being shown in Fig. 12,

Fig. 14.



No. 1. Of the fluorides examined none show any fluorescence, but their absorption-spectra are well worthy of notice. That of the uranous salt is shown in Fig. 13, No. 2, and that of the potassio salt in Fig. 13, No. 1. Uranic formiate gives no fluorescence, and shows an absorption-spectrum with faint bands. The uranic nitrate fluoresces brilliantly, yielding what may be called the normal uranic spectrum. It shows an absorption-spectrum of well-marked, regular bands. All the oxalates yield absorption-spectra, which are very well marked, and seem identical, carrying out the idea above suggested

Fig. 15.



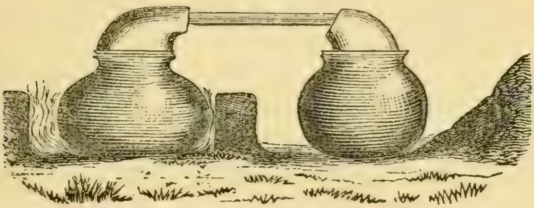
as to the breaking up of double salts when dissolved. The phosphates, like the arseniates, show a remarkable fixity of spectrum, but the absorption-spectra present a variety of forms. The absorption-bands of the neutral and acid sulphates are shown in Fig. 14, Nos. 1 and 2, and in 3 we find the bands of any other uranic sulphate as yet examined. It would seem with the sulphates as with the acetates that all are reduced to the same condition when in solution. Fig. 15 shows the absorption-spectra of the following double sulphates:—(1) ammonio-uranic sulphate; (2) ammonio-diuranic sulphate; (3) magnesio-uranic sulphate; (4) rubidio-uranic sulphate; (5) sodio-uranic sulphate; (6) thallio-uranic sulphate. For a detailed description of the characteristics of the various spectra here mentioned the reader is referred to

the papers as communicated by the authors to the "Chemical News," and which must be regarded as a valuable contribution to chemical and physical science.

TECHNOLOGY.

An instructive paper by Capt. G. A. Strover, of Mandalay, on the Metals and Minerals of Upper Burmah, appeared in the "Chemical News" of October 10. Gold, silver, copper, iron, lead, tin, platinum, graphite, coal, jade and amber, sulphur, saltpetre, rubies, sapphires, garnets, salt, petroleum, india-rubber—all seem to abound. The sulphur is found in efflorescent salts, and is manufactured from metallic sulphurets. The mode of extraction is illustrated below in Fig. 15. Common chatty-shaped vessels are made on the spot from

FIG. 15.

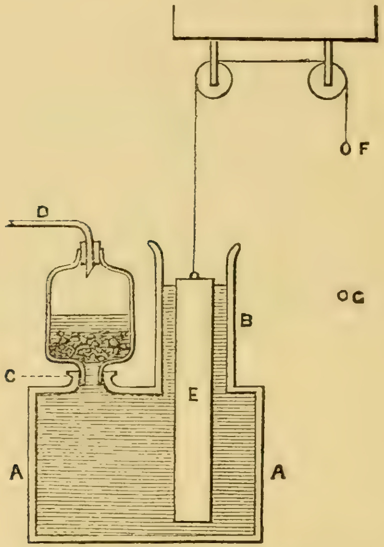


the soft blue clay in which the ore is found. The larger vessel is filled with broken ore, and placed on a fire, a clay retort being fitted to the top, and communicating with the smaller vessel. The sulphur is thus sublimed and condensed as shown, after which the retort is broken, and a hollow tube of flowers of sulphur extracted therefrom which is superior to that condensed in the vessel.

The following improved forms of gas generator were described at the Bradford Meeting of the British Association by Mr. C. J. Woodward, B.Sc.:—Two forms of apparatus had been made. The first, shown in section in Fig. 15, and intended for large supplies of gas, consists of a circular stoneware vessel, A A, holding 3 or 4 gallons, and surmounted by a large cylindrical pipe, B. In the top of the vessel is a tubulure, C, to which is fitted a glass cylinder containing the granulated zinc or other gas-generating material. To the opening of the upper end of the glass cylinder is attached a cork and the delivery tube, D. A plug, E, can be raised or lowered by means of a cord, which, passing over pulleys, terminates in a ring, F. It will readily be seen in what way the apparatus is used. As seen in the figure, the plug, E, is immersed in the acid with which the stoneware jar is filled, and the liquid has risen by displacement into the glass cylindrical vessel, coming in contact with the gas-generating material, where of course the evolution of gas goes on. When it is desired to stop the flow of gas, the plug, E, is raised, the ring, F, being slipped over the stud, G. The acid now retreats from the glass cylinder, and the gas-generating material is left dry. At any time, then, when gas is wanted, it is only necessary to release the ring, F, the plug, E, falls into the acid, the zinc, marble, &c., becomes covered, and the flow of gas begins and can easily be arrested in the manner first described. The second form of apparatus, used when only a small supply of gas is wanted, consists of a Wolff's bottle, A, Fig. 16, to one tubulure of which is fitted a cork carrying a glass tube and piece of caoutchouc piping, B. This pipe, B, can be closed by a pinch-tap, C. To the other tubulure of the Wolff's bottle is fitted the adapter, D, in which is placed the zinc, marble, or other gas-generating material. To the upper end of the adapter is fitted a cork and tube, E, serving for the escape of gas, which is washed at F, and then passes on for use. To use the apparatus, the Wolff's bottle is charged with acid up to the level indicated in the figure. Then, on blowing air by the mouth by means of the tube, B, the pinch-tap, C, being open, acid is forced into the adapter, D, and gas at

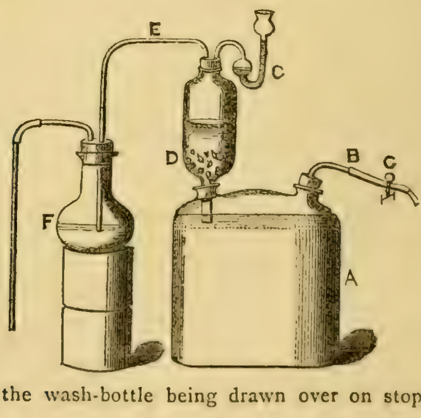
once comes off. The pinch-tap, C, is now closed, and the compressed air in the Wolff's bottle still keeps the acid in the adapter. When it is desired to stop the flow of gas, the pinch-tap, C, is opened, the compressed air escapes,

FIG. 16.



and the acid in the adapter falls, leaving the gas-generating materials dry. Should the blowing of air by the mouth be deemed objectionable, an air-ball may be attached to the pipe, B. A safety-tube, C, is used, in order to prevent

FIG. 17.



the liquid from the wash-bottle being drawn over on stopping the supply of gas.

ERRATA.—Owing to postal delays the following errata did not arrive at the printing office in time for correction :—Page 2, line 3 from top, *for* "external" *read* "nocturnal." Page 5, line 2 from top, *for* "Mars" *read* "mass." Page 35, lines 2 and 18 from bottom, *for* "Jan. 22nd, 1872" *read* "Nov. 13th, 1871."

IMPROVED SPECTRUM APPARATUS.

JOHN BROWNING begs to call the attention of the scientific public to the fact that he has re-modelled and greatly improved nearly the whole of his specialities in Spectroscopes within the last year; more particularly he would mention his MINIATURE SPECTROSCOPE, the DIRECT-VISION SPECTROSCOPE, with Micrometric Measuring-Apparatus, and his UNIVERSAL AUTOMATIC SPECTROSCOPE. JOHN BROWNING can now supply one of these instruments with a dispersive power equal to eleven flint-glass prisms, and so compact that it can be easily adapted to a 4-inch Refracting Telescope.

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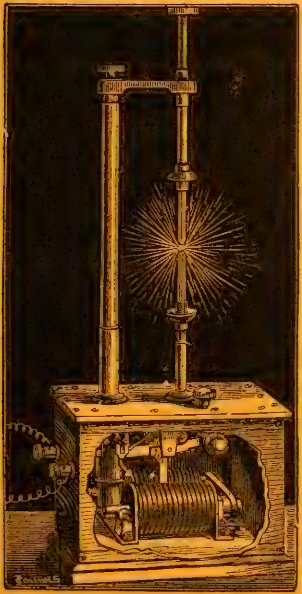

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PROGRESS IN SCIENCE,

(Including the Proceedings of Learned Societies at Home and Abroad, and Notices of Recent Scientific Literature).

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THE QUARTERLY
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I. ON THE FLINT AND CHERT IMPLEMENTS FOUND IN KENT'S CAVERN, NEAR TORQUAY, DEVONSHIRE.

By W. PENGELLY, F.R.S., &c.

ON the northern shore of the beautiful inlet of the English Channel known as Torbay, in Devonshire, stand the town and harbour of Torquay; and about a mile eastward from the harbour there is a small hill, consisting exclusively of limestone, and containing the celebrated Kent's Hole or Cavern. It is but little more than 200 feet above mean tide, whilst immediately on the south-west, and about half a mile to the north-west, rise two loftier eminences, known as Lincombe and Warberry Hills, consisting of grey shales and dark red grits, and reaching the heights of 372 and 450 feet respectively.

Though the cavern seems to have been well known at least three centuries ago, the attention of palæontologists and anthropologists was first directed to it by the labours and discoveries of the late Rev. J. MacEnery, of Torquay. In his "Cavern Researches," he says:—"In the summer of 1825, Dr. Buckland, accompanied by Mr. Northmore, of Cleve, visited the cave of Kent's Hole in search of bones. I attended them. Nothing remarkable was discovered that day, excepting the tooth of a rhinoceros and a flint blade. This was the first instance of the occurrence of British relics being noticed in this, or, I believe, in any other cave. Both these relics 'twas my good fortune to find."*

Towards the close of the same year, he commenced a somewhat systematic exploration of the Cavern, and, in the course of his researches, met with a considerable number of flint implements; and, believing the ground in which they were met with to have been previously broken up, he set himself seriously to work to ascertain whether or not they occurred under the undisturbed floor of stalagmite, covering

* Trans. Devon. Assoc. for the Advancement of Science, Literature, and Art, vol. iii., p. 441. 1869.

the loam or muddy deposit now known as Cave-earth. His description of this work is so graphic, displaying at once his mode of operation and his mental excitement, that I cannot refrain from quoting it :—

"Having cleared away on all sides the loose mould and all suspicious appearances, I dug under the regular [stalagmitic] crust, and flints presented themselves to my hand. This electrified me. I called the attention of my fellow-labourer . . . and in his presence extracted from the red marl arrow and lance-heads. I instantly proceeded to the excavation inside, which was only a few feet distant in the same continuous line, and formed part of the same plate—the crust is about two feet thick, steady (*i.e.*, of uniform thickness); the clay rather a light red. About three inches below the crust, the tooth of an ox met my eye: I called the people to witness the fact, . . . and not knowing the chance of finding flints, I then proceeded to dig under it, and at about a foot I dug out a flint arrow-head. This confirmation—I confess it—startled me. I dug again, and behold, a second! of the same size and colour (black). I struck my hammer into the earth a third time, and a third arrow-head, but white, answered to the blow. This was evidence beyond all question. I then desisted, not wishing to exhaust the bed, but, in case of cavil, leaving others an opportunity of verifying my statements by actual observation."*

The following is the substance of his further remarks on the subject :—The implements lay ordinarily between the bottom of the stalagmite and the upper surface of the cave-earth, but were occasionally, though rarely, buried a few inches deep in the latter, and mixed with the bones of the extinct cave mammals. None occurred in the stalagmite itself. Some were broken, and, as in the case of bones in a similar condition, the separated parts were found at some distance from each other. Instances were met with of indurated or coherent masses made up of Cave-earth, stones, fossil bones, and flint tools. In common with most observers of that time, he ascribed the introduction of the Cave-earth to the Deluge, and though, under the influence of Dr. Buckland, he finally expressed the belief that the implements were "post-diluvial," he contended that the facts just enumerated seemed "to countenance the opposite hypothesis;" though he was staggered by the belief that there were no implements in the cave-earth at depths exceeding a few inches, and no remains of the extinct mammals in the

* Trans. Devon. Assoc., vol. iii., pp. 329-30.

stalagmitic floor. His conviction was decided and firm that before the commencement of the formation of the stalagmite the tools occupied the place in which he discovered them; and when he found Dr. Buckland inclined to attribute them to a more modern date by supposing that the ancient Britons had scooped out ovens in the stalagmite, and that they had thus got admission to the cave-earth, he replied—"Without stopping to dwell on the difficulty of ripping up a solid floor, which, notwithstanding the advantage of undermining and the exposure of its edges, still defies all our efforts, though commanding the apparatus of the quarry, I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam." "It is painful," he adds, "to dissent from so high authority, and more particularly so from my concurrence generally in his views of the phenomena of these caves, which three years' personal observation has in almost every instance enabled me to verify."*

Mr. Godwin-Austen, who, familiar with MacEnery's discoveries, had supplemented them with independent researches of his own, writes thus, in 1840:—"Arrow-heads and knives of flint occur in all parts of the cave, and throughout the entire thickness of the clay; and no distinction founded on distribution or relative position can be observed whereby the human can be separated from the other *reliquiæ*."† It is noteworthy, perhaps, that this important statement entirely removed the chief difficulty which MacEnery felt, in view of the hypothesis that man was, in Devonshire, contemporary with the extinct cave mammals. He believed that the flint tools did not occur at depths of more than a few inches in the cave-earth. It was reserved for Godwin-Austen to discover that, like the fossil bones, they were to be met with at all depths.

In 1846, a committee appointed by the Torquay Natural History Society carried on some very limited but most careful explorations in the Cavern; and make the following statement in their report, drawn up by Mr. E. Vivian, and read, the following year, to the Geological Society of London and the British Association:—"After taking every precaution, by sweeping the surface and examining most minutely whether there were any traces of the floor having been previously disturbed, we broke through the solid

* Trans. Devon. Assoc., vol. iii., pp. 334 and 338.
† Trans. Geol. Soc. of London, (2nd series), vol. vi., p. 444. 1842.

stalagmite in three different parts of the cavern, and in each instance found flint knives. . . . In the spot where the most highly finished specimen was found, the passage was so low that it was extremely difficult, with quarrymen's tools and good workmen, to break through the crust; and the supposition that it had been previously disturbed is impossible."*

Notwithstanding these repeated and concurrent announcements by independent and competent explorers, even the scientific world remained perfectly apathetic or altogether sceptical. Nor were they more influenced by the researches, with similar results, carried on in the caverns near Liège, by Dr. Schmerling, in 1833-34, or those of M. Boucher de Perthes in the river gravels of the Somme, a few years later. This strange apathy appears to have been based partly on the impression that the work had not been executed with sufficient care, and partly on the feeling that the belief respecting the date of the first appearance of man on the earth which held possession of almost every mind neither could nor should be disturbed.

Be this as it may, such was the attitude of most men of science up to 1858, when some quarrymen, in the ordinary course of their work, broke unexpectedly into a virgin cavern on Windmill Hill, Brixham, on the southern shore of Torbay. I almost immediately visited it, prevailed on the proprietor to discontinue the desultory excavations which he had begun, and secured from him the refusal of a lease in the cavern, in the hope that arrangements might be made for making a thorough and systematic exploration. It was soon after visited by the late Dr. Falconer, who, believing it might be capable of throwing light on certain palæontological problems then awaiting solution, prevailed on the Royal and Geological Societies of London to undertake the work. It was entrusted to a committee of the latter body, who placed it under my immediate superintendence as the only member residing in the district. The investigation was begun in July, 1858, and in the following September I was able to announce to the British Association, at Leeds, that in the new cavern flint implements had been found under an unbroken floor of stalagmite, deep in the cave-earth, and mingled with the remains of the ordinary extinct cave mammals. It was at once felt that the method and care which had been observed in the work rendered it impossible to doubt or to ignore the facts, or to resist the

* Report Brit. Assoc., 1847. Proceedings of the Sections, p. 73.

conviction that man had, in Britain, been the contemporary of animals which had become extinct, and that his first appearance was of higher antiquity than had generally been supposed.

In the autumn of 1858, the exploration at Brixham being still in progress, Dr. Falconer, having visited M. Boucher de Perthes, urged Mr. Prestwich to proceed to the valley of the Somme, and make a careful examination of the sections for himself. This was accordingly complied with, and the latter geologist, by embodying the results of his observations in a paper read to the Royal Society in May, 1859, may be said to have completed the revolution. Sir Charles Lyell, when opening the Geological Section of the British Association at Aberdeen, in September of the same year, not only announced his adhesion to the new opinion, but became one of its most powerful advocates; and so great and widely spread was the interest felt in the question that when, in 1863, he published his "Geological Evidences of the Antiquity of Man," he had the pleasure of placing the third edition of his work in the hands of the public before the end of the year.

It being felt to be on many accounts desirable to make a systematic exploration of the large branches of Kent's Cavern still remaining intact, the British Association, when assembled at Bath, in 1864, appointed a large committee to carry on the work, and placed at their disposal a considerable grant of money for the purpose. The investigation, under the immediate superintendence of Mr. Vivian and myself—the only two resident members of the committee—was commenced on 28th March, 1865; it has been continued to the present time, and is still in progress. Nine annual reports have been sent in to the Association and ordered to be printed, and ample grants of money have been voted every year.

It is my object in the present paper to state what stone implements have been discovered in the cavern, and to call attention to the fact that whilst all the noteworthy specimens are unpolished, and found with the remains of extinct mammals, they belong to two distinct classes, eras, and stages of civilisation.

Though there are said to be persons capable of believing that the so-called stone implements found in Kent's Hole and other caverns, as well as in the river-gravels, are merely natural products, it is not my intention to say one word on that question. It has been treated so fully and so ably by various writers as to deprive me of any pretence for attempting

to add anything to the literature of the subject, and also of any hope that such additions as I might be able to make would have the least effect on those still remaining in a sceptical state.

It may be as well at the outset to describe the successive deposits, and their principal contents, met with in the cavern during the exploration now in progress. They are as follow, in descending order :—

1st, or uppermost. Blocks of limestone, from a few pounds to upwards of 100 tons each, which had fallen from the roof from time to time, and were in some instances cemented together with carbonate of lime.

2nd. Beneath and between the blocks just mentioned lay a dark-coloured mud, consisting largely of decayed leaves and other vegetable matter, from 3 to 12 inches thick, and known as the *Black Mould.*

3rd. Under this was a stalagmitic floor, commonly of granular texture, varying from an inch or even less to upwards of 5 feet in thickness, frequently containing large blocks of limestone, and termed the *Granular Stalagmite.*

4th. An almost black layer, about 4 inches thick, composed mainly of small fragments of charred wood, and distinguished as the *Black Band*, occupied an area of about 100 square feet, immediately under the granular stalagmite, and, where nearest to it, about 32 feet from one of the entrances to the cavern. Nothing resembling it was found elsewhere.

5th. Immediately under the Granular Stalagmite and the Black Band, lay an accumulation of light red clay, containing on the average about 50 per cent of small angular fragments of limestone, and somewhat numerous blocks of the same materials as large as those already mentioned as lying on the Black Mould. In this deposit, known as the *Cave-earth*, many of the stones and osseous remains were, at all depths, invested with thin stalagmitic films; and it occasionally contained large isolated masses of stalagmite having a very crystalline texture, sub-angular and rounded fragments of quartz and dark red grit sometimes cemented into more or less round detached lumps of firm concrete, and a very few granitoid pebbles. The Cave-earth was usually of unknown depth, certainly and perhaps greatly exceeding 4 feet, but it was occasionally much less, and in some instances there was none.

6th. Wherever the bottom of the Cave-earth was reached, there was found beneath it a floor of stalagmite having a

crystalline texture, identical with that of the isolated masses just mentioned as being incorporated in the Cave-earth. This, designated the *Crystalline Stalagmite*, was usually of greater thickness than the upper or granular floor, in the same branch of the Cavern, and was in some instances but little short of 12 feet. Where there was no Cave-earth, the two stalagmites lay one immediately on the other.

7th. Below the whole occurred, so far as is at present known, the lowest and oldest of the deposits which the Cavern now contains. It was composed of sub-angular and rounded pieces of quartz and dark red grit, the latter being the more prevalent, embedded in a sandy paste of the same colour. Small angular fragments of limestone, and thin investing films of stalagmite, both prevalent in the Cave-earth as already stated, were extremely rare; large blocks of limestone were occasionally met with, and the deposit, to which the name of *Breccia* has been given, was of a depth exceeding that to which the exploration has yet been carried.

The masses of Crystalline Stalagmite and the fragments and lumps of dark red grit found embedded in the Cave-earth are undoubtedly portions, not *in situ*, of the two older deposits—the Crystalline Stalagmitic floor and the Breccia—and show that these accumulations had been broken up by some natural agency at least partially before the introduction of the Cave-earth into the Cavern, and that they were formerly of greater volume than at present.

Excepting the overlying blocks of limestone (No. 1), which need not be mentioned again, all the deposits contained remains of animals. In the Black Mould, the most modern accumulation, they were those of species still existing, and almost all of them now occupying the district. They were man, dog, fox, badger, brown bear, *Bos longifrons*, roe-deer, sheep, goat, pig, hare, rabbit, water-rat, and seal.

In the Granular Stalagmite, Black Band, and Cave-earth, extinct as well as recent species presented themselves. The cave-hyæna was the most prevalent, but was followed very closely by the horse and rhinoceros. Remains of the so-called Irish elk, wild-bull, bison, red-deer, mammoth, badger, the cave, grizzly, and brown bears, were by no means rare; those of the cave-lion, wolf, fox, and rein-deer were less numerous; and those of beaver, glutton, and *Machairodus latidens* were very scarce. The presence of the hyæna was also announced by his coprolites, by bones broken after a manner still followed by existing members of the same genus, and by the marks of his teeth found on a very large proportion of the osseous remains.

In the lowest deposits—the Crystalline Stalagmite and the Breccia—remains of animals were less uniformly distributed. In some instances there were none throughout considerable volumes of the deposits, whilst in others they were so numerous as to form 50 per cent of the entire deposit. To use the language of one of the workmen employed in the exploration, "they lay about as thick as if they had been thrown there with a shovel." So far as is at present known, they were exclusively the remains of bears. Not only were there no bones of the hyæna, there were none of his fæces, none of his teeth-marks, no bones fractured according to his well-known pattern,—nothing whatever to indicate his existence.

The bones found in the uppermost deposit—the Black Mould—were of much less specific gravity than those in the accumulation below it, and were generally so light as to float in water. Those in the two sets of deposits represented by the Cave-earth and the Breccia respectively, had lost their animal matter, and adhered to the tongue when applied to it so as frequently to support their own weight; but those from the Breccia and its Stalagmite—the lowest deposits—were distinguished from those of the Cave-earth series in being much more mineralised, more brittle, and by frequently emitting a metallic sound when struck.

The following general statements may be of service, by way of recapitulation, before proceeding further:—

1st. The Cavern contained three distinct mechanical accumulations—the Black Mould, or uppermost, or most modern; the Cave-earth, including the local Black Band; and the Breccia, or lowermost, or most ancient. Their mode of succession was never transgressed, and the materials of which they consisted were so very dissimilar as to characterise them with great distinctness.

2nd. These three accumulations were separated by two distinct floors of stalagmite having strongly contrasted characters. That dividing the Black Mould, or uppermost deposit, from the Cave-earth was granular; whilst that lying between the Cave-earth and Breccia, or lowermost deposit, was eminently crystalline.

3rd. Animal remains occurred everywhere, but were much more abundant in the mechanical deposits than in the stalagmites.

4th. The period represented by the Black Mould—the most modern period—may, as a matter of convenience, and so far as the Cavern is concerned, be termed the *Ovine period*, remains of sheep being restricted to this accumulation.

5th. The period of the Granular Stalagmite, Black Band, and Cave-earth may be denominated the *Hyænine period*, the remains of hyæna being confined to these deposits, and more prevalent there than those of any other species.

6th. The period of the Crystalline Stalagmite and Breccia —the most ancient period represented by the Cavern deposits so far as they are at present known—may be called the *Ursine period*, these deposits having yielded remains of bear only. It must be understood, however, that bears are represented in all the deposits.

7th. The bones of each period were distinguishable by their condition; those from the Black Mould being lighter, and those found in the Breccia being more mineralised, than the products of the Cave-earth.

Flint and chert implements presented themselves in each of the mechanical deposits, and, as in the case of the bones, those belonging to any one of them were easily distinguishable from those of the other two.

The implements of the Black Mould—the Ovine, or most modern period—were of the ordinary colour of common flints. They were mere flakes and "strike-lights," the latter probably used and cast aside or lost by those who, during a long period, and before the invention of lucifer-matches, acted as guides to the Cavern. All further mention of them may be omitted as not being noteworthy.

Omitting mere flakes and chips, of which there were great numbers, the principal implements found in the Cave-earth—the Hyænine period—were ovoid, lanceolate, and tongue-shaped, produced by fashioning, not flint or chert nodules, but flakes struck off them for the purpose. They were of comparatively delicate proportions; those of flint were usually of a white colour and porcellanous aspect, and having, through metamorphosis, a granular chalk-like texture.

These are not the only human industrial remains found in the Cave-earth, as it has yielded a bone needle or bodkin with a well-formed eye, three bone harpoons or fish spears—one of them barbed on both sides, and the others on one only—a bone pin, a bone awl, hammer stones, "whetstones," charred bones and wood, and a badger's canine tooth having its fang artificially perforated apparently for the purpose of being strung with other objects to form a necklace or bracelet—an indication that the cave men of the Hyænine period occupied themselves in making ornaments as well as objects of mere utility.

The implements from the Breccia—the Ursine, or most ancient of the periods—were much more rudely formed,

more massive, less symmetrical in outline, and made by operating, not on flakes, but directly on nodules, which appear to have been derived from supracretaceous gravels between Torquay and Newton Abbot, about four miles from the Cavern, and generally retain portions of the original surface. It is obvious, however, that even such tools could not be made without the dislodgement of flakes or chips from the nodule, and, accordingly, remnants of this kind have presented themselves in the Breccia, but they are all of a very rude character. There was also a mass of flint which may have been a "core" from which flakes had been struck, or, what seems not less probable, the useless result of an abortive attempt to make a tool.

No such implements have been found in the Cave-earth, nor have any of the comparatively slender, symmetrical tools of this less ancient deposit been found in the Breccia. They are by no means so abundant as those of the Cave-earth; that is to say a given volume of Breccia has not yielded so many implements as would on the average occur in an equal volume of the more modern accumulation. Whether equal periods of time are represented by equal volumes of deposit in the two cases, or whether equal periods of time represent equal numbers of human cave dwellers or tool-makers in the two eras, are questions which present themselves, but into which it is not possible to go fully at present. Omitting rude flakes and chips as well as the "core" or "failure" just mentioned, the Breccia has up to this time yielded no more than eleven specimens of the kind. It must be remembered, however, that the time during which the explorers have been excavating the Breccia is comparatively short. The implements just mentioned are the only indications of man which have been found belonging to the Ursine period.

That the implements from the Breccia belonged to an earlier period than those from the Cave-earth is obvious from the position they occupied; they were lodged in a deposit which, when the two were found in the same vertical section, invariably underlay the Cave-earth. In fact, every tool found in the Breccia actually had Cave-earth above it.

That the two periods were separated by a great chronological interval is indicated by the geological and palæontological facts, and the considerations growing out of them:—

1st. The Breccia and the Cave-earth, though deposited on the same area, were very dissimilar, as has been stated; the former consisting of a dark red sandy paste containing a very large number of sub-angular and rounded fragments of dark

red grit, which, though derivable from the adjacent and loftier eminences, the Cavern-hill could not supply; and the latter, or more modern, being made up of a light red clay with small angular fragments of limestone.

2nd. These two deposits were separated by a sheet of Crystalline Stalagmite, in some places almost 12 feet thick, formed after the materials of the Breccia had all been introduced, but before the introduction of the Cave-earth commenced.

3rd. After the Stalagmite just mentioned had sealed up the Breccia, it was in extensive parts of the Cavern, broken up by some natural agency, and much of the underlying Breccia was dislodged and carried out of the Cavern, before the first instalment of Cave-earth was deposited.

4th. The Cavern faunæ during the two periods were very dissimilar: that of the Breccia did not include the hyæna, which played so conspicuous a part in the Cavern history during the subsequent Cave-earth era, and whose agency, next to that of man, made cavern searching an important branch of science. When his cavern-haunting habits are remembered, it will be seen that his absence in the one fauna, and his presence in the other, render it eminently probable that he was not an occupant of Britain during the earlier period.

The same inference cannot with an equal approach to certainty be drawn respecting the horse, ox, deer, &c., though the absence of their remains from the Breccia is equally pronounced; for it may be presumed that their bones occur in caverns simply because their dead bodies were dragged there piecemeal by the hyæna, and this could not have occurred, even though they had crowded the country, before the arrival of the great bone-eating scavenger who made the Cavern his home.

The remains of the bear in the Breccia present no difficulty, for their introduction did not require the agency of the hyæna, since the bear is a cave-dweller. Dr. Leith Adams, F.R.S., so well known as a naturalist and cavern explorer, writing me on the subject, says:—"The Brown Bear of the western Himalayas hybernates, choosing chiefly caverns and rock crevices, which it abandons in spring to wander about; but old individuals, when no longer equal to the same amount of exertion, take to a secluded life, and usually select a cavern on a rocky mountain side, at the base of which there is abundant verdure and shade, with a pool or spring where they bathe frequently, or recline during the heat of the day to escape the annoyance of insects. Such

retreats are easily discovered by the animals' footprints on the soil and turf. They are seen like steps of stairs leading from the pool in the direction of the den, being brought about by the individual always treading in the same track. Thus the patriarch or hermit bears spend their latter years in one situation, pursuing the even tenor of their ways to the little stream or pond below, and grassy slopes, to feed on the rank vegetation, returning regularly to the caverns, where they end their days."

5th. The deposits just described are obviously not only distinct, successive, and protracted terms in the Cavern chronology, but indicators of changes in the conditions of the geographical features of the immediately surrounding country, and of the relation of the Cavern to it. During the period of the Breccia there was a machinery capable of transporting from Lincombe hill, or Warberry hill, or both, or from some greater distance, fragments of dark red grit, varying in size from pieces four inches in mean diameter to mere sand, and lodging them in the Cavern. This so completely passed away that nothing was carried in; but the deposit, already there, was covered with a thick sheet of Crystalline Stalagmite obtained through the solution of portions of the limestone in the heart of which the Cavern lay. This also ended: the stalagmite was broken up by some natural agency, apparently not by one effort, but by many in succession, and much of the Breccia was dislodged and carried out of the Cavern. This having in like manner come to a close, again a deposit was introduced; but, instead of being dark red stones and sand, as in the former instance, it was a light red clay; and in it were embedded small fragments of limestone, which, from their angularity, could not have been rolled, but were in all probability supplied by the waste of the walls and roof of the Cavern itself. It contained also the bones of numerous species of mammals, and of these the remains of the hyæna were the most prevalent.

Nor is the palæontology of the two periods less significant of physical charges. If the absence of any traces of the hyæna from the older deposit has been correctly interpreted above, as signifying that the species was not then an occupant of Britain, it follows that it was subsequently possible for him to arrive here, or, in other words, Britain had become connected by dry land with the Continent. In short, the facts point to the conclusion that the earliest Devonshire men known to us—the men of the Ursine, the Breccia period—saw this country an island as we see it, and that in

the time of their descendants, or their successors, prior to the Hyænine or Cave-earth era, it had reached a continental condition. This latter condition has so long ceased that the earliest traditions respecting our land recognise it as an island, even though they profess to go back to a time when Anglesea was not yet detached from Wales.*

Readers of Sir C. Lyell's "Antiquity of Man" are aware that he recognises two distinct periods in geologically recent times when Britain was in a continental condition. He supposes that the well-known "forest of Cromer," in Norfolk, in which Scotch and spruce first were prevalent, flourished at the close of the first of them; that in the intervening period, the land north of the Thames and Bristol Channel was gradually submerged until a few mountain tops alone remained above the sea; and, from the evidence then available, that the first appearance of man when . . . . he ranged from all parts of the Continent into the British area, took place during the second continental period."†

If, however, the new evidence from Kent's Cavern has been correctly interpreted above, the first appearance of Man in Britain was prior to the second continental period, and must have been at least as early as the previous insular era. Indeed, unless we suppose him to have possessed the means of navigation, it must have been in the first continental period.

It is worthy of remark that the hypothesis that the land south of the Thames and Bristol Channel was not submerged during the interval separating the two continental periods, harmonises well with the supposition that Devonshire was occupied by man during that interval.

Without at present attempting to pursue further the question of the relation of the oldest Devonshire men yet known to Glacial times, I cannot divest myself of the belief that the complete exploration of Kent's Cavern will furnish a definite reply to it.

Though no one acquainted with the present state of the evidence would attempt to express in years, or other astronomical units, the amount of time represented by the deposits of the Ovine and Hyænine periods, it cannot be doubted that the spindle whorls, the pottery, the bone combs, and the fibulæ of the Black Mould—the first or uppermost of these—go back to Romano-British and Pre-Roman times, to at least two thousand years from the present day as a minimum. All that is known about the Granular Stalagmite—

* See the 67th of the Historical Triads of Britain.

† Antiquity of Man, pp. 282-3.

the preceding term of the series—points to the conclusion that it was formed slowly. That the main lines of drainage through the roof have remained unaltered is seen in the facts that those parts of the cavern which have an unusually thick floor of stalagmite have also, at present, more than an ordinarily copious drip; where the floor is but thin the drip is never of great amount; and where there is no floor—for a few such places occur—the cavern is quite dry at all seasons; and further, wherever a conspicuous stalactite depended from the roof, there was found, vertically below, rising from the Granular Stalagmitic floor a considerable boss of the same material to meet it; and whenever the lower or Crystalline Stalagmitic floor was found in such a section, a similar but larger boss existed on it also. In several parts of the Cavern there are names, initials, and dates inscribed on the Granular Stalagmite, where it is certainly of great thickness, and where additions are still being made to it. There is satisfactory evidence of their genuineness; and, though some of them are upwards of two hundred and fifty years old, and are slightly glazed over, they are perfectly legible, and the film accreted on them cannot be more than the twentieth of an inch in thickness. It is not pretended, of course, that this rate *must* be taken as a trustworthy chronometer for the entire thickness, but those who object to it must expect to be called on to state why they do so; and even if the objection should be sustained, it will be seen that, when a thickness of five feet presents itself for measurement—a rate even ten times as great as that which has certainly obtained for more than two and a half centuries—betokens a great antiquity.

That the Cave-earth, every portion of which is necessarily older than the most ancient part of the stalagmite covering it, was accumulated very slowly is seen in the great number of small angular fragments with which the walls of the cavern have crowded it, and in the films of stalagmite which, as already stated, invest the bones and stones everywhere throughout the total depth of the mass, since each such film indicates that the spot the invested object occupies was a portion of the floor of the Cavern during a time sufficient for its accretion, and that it was only prevented from growing into a thick wide-spread sheet by the introduction and lodgement on it of a small instalment of Cave-earth.

Whatever be the aggregate amount of time represented by the less ancient deposits just spoken of, there can be little doubt that at least fully as much is absorbed by the more ancient cavern history, of which the formation of the Breccia and Crystalline Stalagmite, as well as the subsequent

destruction and dislodgement of great portions of these, are the exponents. It may be true that the Breccia was introduced at a more rapid rate than the Cave-earth; and, indeed, this is rendered not improbable by the great paucity of angular fragments of limestone, as well as of films of Stalagmite in the older deposit; but this is probably more than neutralised by the immense thickness of the Crystalline or older Stalagmite as compared with that of the Granular or more modern; the former being in one chamber but little short of 12 feet, whilst the latter has in no instance much exceeded 5 feet.

In short, and speaking for myself, however far back in antiquity the fabricators of the Cave-earth tools take their stand, I cannot hesitate to place those of the implements of the Breccia as much further back. Many must remember, and perhaps few were surprised at, the alarm occasioned by the antiquity of man disclosed by the researches in Brixham Cavern, in 1858; and now, cavern researches, growing out of those just mentioned, appear to me to make an irresistible demand to have human antiquity in Britain at least doubled.

Up to the present time, as the Cavern has disclosed more and more ancient men, it has shown that they were ruder and ruder as they withdrew into antiquity. The men of the Black Mould had a great variety of bone implements, they used spindle whorls, and made pottery, and smelted and compounded metals. The older men of the Cave-earth made a few bone tools, they used needles, and probably stitched skins together, and even perforated badgers' teeth to enable them to be strung as necklaces or bracelets, but they had neither spindle whorls nor pottery, nor metals of any kind; their most powerful weapons were made of flakes of flint and chert, many of them symmetrically formed and carefully chipped, but it seems never to have occurred to them to increase their efficiency by polishing them. The still more ancient men of the Breccia have left behind them not even a single bone tool; they made implements of nodules, not flakes, of flint and chert; tools that were rude and massive, had but little regularity of outline, and were but roughly chipped.

It has not been unusual to hear the men of the Cave-earth period spoken of as *Primeval Men*, or *Aborigines of Devonshire;* the discovery of men of higher antiquity in the same area is at once a proof that the names are inappropriate, and a warning against applying them to even the men of the Breccia; for, though these are no doubt the oldest men yet known to us, they hint that further discoveries may yet be made.

II. ON RECENT EXTRAORDINARY OSCILLATIONS OF THE WATERS IN LAKE ONTARIO AND ON THE SEA-SHORES OF PERU, AUSTRALIA, DEVONSHIRE, CORNWALL, &c.

By RICHARD EDMONDS.

THESE tide-like alternating currents, which resemble in all respects those observed on the shores of this and other countries on the days of the two great earthquakes of 1755 and 1761, commence generally, if not always, with an efflux, and occupy from five to ten minutes in ebbing, and about the same time in flowing, the ebb and flow and the imperceptible interval between them never exceeding twenty minutes. Those on the 5th of July, 1843, were observed between the hours of 11 a.m. and 5 p.m. in Plymouth, Falmouth, Mountsbay, the Scilly Isles, Bristol, and on the eastern coast of Scotland at North Berwick and Arbroath. Those of the 29th of September, 1869, occurred on the northern as well as on the southern coasts of Devonshire and Cornwall, and at the Scilly Isles. Of these and all the intermediate ones of importance in these two counties I have written full descriptions, including all meteorological particulars.*

These currents, when running in and out of piers or narrow channels, often drive vessels from their moorings, and dash them against one another. But when they are moving up and down a wide open beach, the motion is so quiet and tide-like that they would not be perceived unless they were watched for some time. This was amusingly exemplified at Penzance, when, during such an oscillation, some children who had been long enough on the beach to discover it made a play of their discovery, by inducing other children who had not observed it to go out on some rocks left dry by an efflux, where they were soon surrounded, and for many minutes in a great fright lest they should be drowned.

Such is the nature of these extraordinary oscillations *after the first efflux*, whether on sea-shores or on the shores of lakes. The *first efflux* is well illustrated by that in Lake

* See "Literary Gazette" of June, 1843; The "Edinburgh New Philosophical Journal," 1845, and following years until it merged into the "Quarterly Journal of Science;" and the "Philosophical Magazine" for January, 1866, and January, 1869.

Ontario on the 13th of June, 1872, as related in the "Rochester Democrat" of the 15th of that month. While some gentlemen of Rochester were in a boat near the beach, "where the water is usually 2 feet deep at least, their boat suddenly grounded, and the waters receding left her on a sand-bank. The gentlemen got out and strolled away, but looking back shortly after, they saw to their surprise the boat dashing about in apparently deep water. Securing the boat with some difficulty, they found her suddenly aground again, and as suddenly floated after a short interval. Becoming now interested in this curious ebb and flow of the lake, they diligently observed it for about three hours. *The ebb and flow occurred every twenty minutes, that is, for ten minutes the water would gradually recede, then commence rising, and continue to rise for about ten minutes. The water rose 2 feet and 3 or 4 inches above the ordinary level, then receded about the same distance below the usual level, making a variation in the height of the water of nearly or quite 4½ feet every twenty minutes.*" The above quotation is from the "Croydon Chronicle" of July 13, 1872, and what I have italicised is also a quotation from an abbreviated notice in the "Times" of the same date.

There was a similar occurrence in this lake on the 20th of September, 1845, when the waters suddenly moved "in a mass out of the rivers, bays, coves, harbours, &c.," to a depth of 2 feet, and then returned to an equal height above their previous level. This happened on each side of the lake.*

The explanation of these phenomena, whether in lakes or on the shores of the sea, I have given in the "Transactions of the Royal Geological Society of Cornwall" for 1843, and as it is the only one yet proposed that can reconcile all the observed facts connected with them, I will now give it more fully, and with the addition of some recent confirmatory facts.

My hypothesis is that an earthquake shock proceeding upwards vertically from the interior of the earth reaches the basin of the lake. Now wherever this basin is horizontal the shock continues its vertical ascent up through the water as through a solid body, and with greater velocity than the most rapid flight of a cannon-ball; but none of the water is thereby displaced except the surface, which, as will be presently exemplified, is dashed up vertically and then falls back into its previous place, no current being produced. If a ship were on the spot, it would receive the shock, and

* Edinburgh New Phil. Journal for July, 1848, pp. 107-109.

loose pieces of timber, or anchors lying on the deck, and even men standing on the deck, as will also presently be shown, would be jerked up to heights proportioned to the violence of the shock. Much of the basin of the lake, however, is not horizontal, but sloping from its shores. Therefore when the shock which had proceeded vertically up from the interior of the earth reached the inclined or sloping sides of the basin, it would not continue to proceed up through the incumbent water vertically or perpendicularly to the horizon, but perpendicularly to the inclined plane of the basin, whereby the surface water dashed off would be jerked towards the centre of the lake. What I have called an earthquake-shock is really, however, only a single vibration of an earthquake-shock; for an earthquake-shock consists usually of a rapid succession of vibrations, sounding at *sea* "like the letting out of a cable," and on *land* "like a waggon rushing over a paved road." By such vibrations, therefore, from the sloping sides of the basin, even if there were only five or six in a second, and the shock lasted only 30 seconds, a great heap of water would be raised. The first vibration would, as I have said, jerk the surface water towards the centre of the lake, and before this dashed off surface water had time to flow back to its place, fresh surfaces would be jerked in the same direction by succeeding vibrations, so that the successive surfaces, whilst being thus dashed off, would form a current towards the centre, to supply which an efflux from the shores would necessarily follow. This efflux would not cease until the vibrations ceased, and then the heaped-up waters would immediately flow back to the shores. The subsequent ebbings and flowings, like the oscillations of a pendulum, would continue until the equilibrium were restored.

The extraordinary movements of the waters so often observed in the American lakes, though not always of the same description as the two in Lake Ontario above given, may all, I think, be accounted for by my hypothesis. For example, if the earthquake shock in the bed of the lake be very partial, and do not occur on its sloping sides, but only on shoals near its centre—say two hills, or two parallel ridges, with sides inclining down into the intermediate valley—then the waters driven from these opposite sides would meet over the valley and form a high wave, which might flow on to the shore and rush up without any previous efflux beyond that slight and momentary retirement of the water which always precedes the fall of a wave on the shore.

I have said that a vertical shock proceeding from a

horizontal portion of the basin of a lake occasions no current, because the surface of the water is thereby jerked up in jets perpendicular to the horizon, which jets fall back into the places they had previously occupied. An illustration of this appears in the following extract from the "Times" of October 18, 1869, describing the effects of a vertical shock from a horizontal portion of the bed of the sea, over which a ship was then passing:—"A correspondent writing from Valparaiso, on 3rd September, 1869, says, 'I arrived here on the 28th ult, in the steam ship "Payta," from Callao. . . We left Arica in the morning of the 24th ult., and at 1.20 p.m. (the "Payta" being two to two and a half miles from the land, fifty seven miles south of Arica, off Gorda Point, and in seventy-five fathoms of water), the vessel commenced to shake in the most awful manner, making it impossible to keep one's footing. (In another account in the same day's paper by the purser of the "Payta," it is stated that 'the vibration threw the passengers to the height of two inches* above the deck'). This convulsed vibration continued 45 or 50 seconds, during which time the sea presented the appearance of a heavy fall of large hailstones, *sending up jets of water* some eight or ten inches, and being about the same distance apart. On arriving at Iquique the same night, we learnt that two very severe shocks of earthquakes had been felt there about two o'clock in the day." Had this vertical shock off Gorda Point proceeded from an inclined shore, the jets of water, instead of being jerked up vertically, and falling back into their previous places as they did, would have been jerked up obliquely in a seaward direction, rising so little above the surface that the separate jets would not be distinguishable, and the entire surface would have been seen as a vast current rushing *seaward*, so long as the vibrations continued; and, when they ceased, the heaped up water would return *shoreward* to find its level.

We have seen that in Lake Ontario, in 1845, as well as in 1872, the water first retired to a depth of two feet or more, and then returned to an equal height above its previous level, just as a *harp-string or pendulum, when moved to any distance from its point of rest, will immediately of itself move twice such distance in the opposite direction.* This applies to marine as well as to lacustrine disturbances. Luke Howard, describing those at Plymouth on the 11th of May, 1811, says: "the sea fell instantaneously about four feet, and

* Lyell records an instance of men on board a ship forty leagues west of Cape St. Vincent being thrown by an earthquake shock "a foot and a half perpendicularly up from the deck."

immediately rose about eight feet." Those "mountainous" waves, therefore, which overwhelmed the Peruvian cities of Iquique and Arica, on the 13th of August, 1868, were nothing more than might have been anticipated from the illustration by the harp-string and pendulum. For the great wave, *forty feet high*, which completed the destruction of Iquique (directly after the earthquake shock had laid it in ruins) was immediately preceded by a retirement of the sea to the extraordinary depth of *four fathoms*. It was by a still higher wave, arising from an apparently still greater retirement of the sea, that Arica was destroyed, as appears from the following account given by Mr. Nugent, H.M. Vice Consul, in the "South American Missionary Magazine" for October, 1868. "In the afternoon of August 13, 1868, about 5 o'clock, we were visited with a most tremendous earthquake. I had scarcely time to get my wife and children into the street, when the whole of the walls of my house fell, or rather were blown out, as if *jerked* at us. I started over the *trembling ground* for the hills. . . . A great cry went up to heaven, such as few men have heard, 'The sea is retiring.' . . I looked back. . . . I saw all the vessels in the bay carried out irresistibly to sea (anchors and chains were as packthread), probably with a speed of ten miles an hour. *In a few minutes the great outward current stopped, stemmed by a mighty rising wave*, I should judge about *fifty feet high*, which came on with an awful rush, bringing in the whole of the shipping with it, sometimes turning in circles as if striving to elude their fate. . . . The wave passed on, struck the mole into atoms, swallowed up the Custom-House, . . . carried everything before it. In a few minutes all was completed—every vessel was either ashore or bottom upwards." "*The mighty rising wave*" here described was evidently the accumulation produced by "*the great outward current*," and that *outward current* was caused by "*the trembling ground*" beneath the sea, that is by the rapid vibrations of the inclined bed of the sea descending from the shore; for as the dry land was then "*trembling*" or vibrating by the vertical shock, the adjoining submarine ground must have been so also. This is a fair specimen (although on a terribly large scale) of all those extraordinary agitations now under consideration.

Mr. Mallet, however, entertains the idea proposed by Michell a century ago, that these extraordinary *influxes* proceed from the offing, and from vast but very low waves formed by submarine earthquakes or volcanos at the distance

sometimes of thousands of miles,* instead of their being merely the reaction, the rebounds or *refluxes* of the immediately preceding *effluxes* occasioned, as I have shown, by *local* submarine earth-*shocks*. And writers in some of our leading periodicals†, having adopted the same idea, have ascribed the extraordinary disturbances of the Sea in California, New Zealand, Australia, and other islands in the Pacific, on the 13th and 14th of August, 1868, immediately after the destruction of Arica, to one of these great sea waves produced by a submarine earthquake near Peru, and moving onwards in all directions with varying velocity (like other great sea waves) according to the square roots of the varying depths which they traversed. But such waves are only imaginary; they have never been seen, nor can their existence be proved. None such existed at any of the similar agitations in the West of England which I have described during the last thirty years; and, therefore, none such can be rightly assumed to have existed at any other similar agitations. To remove all doubt, however, on this point, I invite attention to what was observed at Plymouth, on the 29th of September, 1869, during the extraordinary disturbances of the sea that day, on the northern as well as on the southern coasts of Devonshire and Cornwall. *Ex uno disce omnes.*

The alternating current in Cattewater, an anchorage outside Plymouth pier (Sutton Pool), rushed in and out of the pier every 15 or 20 minutes, whirling about the boats and vessels, parting their hawsers, and carrying a schooner which had almost reached the pier back two or three furlongs, and then to and fro so helplessly that a steam tug was despatched to her assistance, and towed her in. This commotion was almost universally believed to have come in from the offing, through the two entrances at the ends of the breakwater. I was sure from past experience that it did not, and so it proved. One of the keepers of the lighthouse on the breakwater, and one of the labourers there, informed me that they were on the breakwater all that day, and that the sea there was quite undisturbed. Moreover, the foreman of the labourers, who was absent that day, told me that, after having witnessed the extraordinary disturbance in Cattewater, he was so convinced of its having proceeded from outside the breakwater, that he fully expected, when he

* Quarterly Journal of Science for January, 1864.

† See the articles on Earthquakes in the "Times" of November 3, 1868, and the "British Quarterly Review" for January, 1869, and "Blackwood's Edinburgh Magazine" for July, 1869.

went thither, to find all his recent unfinished work washed away. But on arriving at his work the following day, and finding it precisely as he left it, and hearing from his men that the sea had been all the time of his absence quite undisturbed, this unexpected information was to him just as marvellous as the phenomenon itself in Cattewater.

The only possible cause of the disturbances of the sea this day, on the northern as well as on the southern coasts of Devonshire and Cornwall, and at the Scilly Isles, appears to be local submarine vertical shocks, not rising higher than the bed of the sea.

These phenomena are never, as I consider, occasioned by *undulatory* earthquakes, but only by vertical shocks. During the earthquake at Antioch, on the 3rd of April, 1872, "so long as the *undulatory* motion continued, no houses fell, but as soon as *vertical* jerks set in, a large part of the town was in a few seconds a heap of ruins."* Providentially these vertical shocks, proceeding from the deep interior of the earth, do not generally rise higher than the basins of lakes, or the bed of the sea. Thus, on the day of the great earthquake of 1755, whilst only one shock was perceived on the surface of the mines in Derbyshire Peak, five smart ones were felt sixty fathoms underground†. On the same day, not only Lochness and other lakes in Scotland, but even ponds in England, were violently agitated without any perceptible shock in their neighbourhoods.

III. THE NATIVE COPPER MINES OF LAKE SUPERIOR.

By James Douglas, Quebec.

THE Jesuit fathers who, in extending the domain of Christianity two centuries ago, explored and described parts of the American continent, which are still almost as wild as then, likened Lake Superior to a relaxed bow on whose string rests an arrow, the north or Canadian shore being the bow, the south or United States shore the bow-string, and the arrow the promontory of Keweenah, which, protruding from the south shore far across the lake, divides its waters almost into halves. This promontory,

* Times of July 30, 1872.
† Phil. Trans., 49, p. 398.

while one of the most salient geographical features of the lake, is moreover geologically and mineralogically the most remarkable, for on it, running from end to end, exist in their greatest development those cupriferous beds of trap and conglomerate in which native copper occurs under conditions most puzzling to the mineralogist, and from which it is being extracted in quantities sufficient to supply the growing wants of the United States and to threaten the stability of the copper market elsewhere.

In the present article, it is not my object to discuss the cosmical bearing of the subject, but to describe two of the most noted mines near Portage Lake and the means adopted to extract the mineral from their ores. Nevertheless, a sketch of the geology of the region and of the mining elsewhere in it is necessary as a preface. Lake Superior is framed in primitive rocks. The gneisses and granites of the Laurentian formation at places rise in bold cliffs out of the waters along the east and north shores, and where the shore line in its trend to the south-west leaves the Laurentides, the intervening space is occupied by a narrow belt of Huronian slates and conglomerates, on which seem to rest unconformably, judging from the scanty evidence afforded by the survey of this part of the north shore, but conformably, according to Brookes and Pompelly,* who have examined the lines of contact on the south shore, a series of beds of bluish shale, sandstone, indurated marls and conglomerates, interstratified with trap, which is sometimes amygdaloidal.

Sir William Logan subdivides this great mass of rock, whose total thickness can be but vaguely guessed at, into lower and upper groups, and designates them as the upper copper-bearing rocks of Lake Superior, in distinction to the Huronian or lower copper-bearing series.

The lower group occupies the north-western shore of the lake, and sweeps round its extreme westerly end, but in the extension eastward of it and the upper group they are divided from the south shore by sandstones of a very different character to those which are interstratified with their own traps and conglomerates. These sandstones, which line the south shore, with but few interruptions, from Sault St. Marie to Duluth, lie in horizontal or very slightly inclined beds, and, being very friable, have been at several spots fashioned by the water into the fantastic forms known as the pictured rocks. Representatives of the same sandstone

* American Journal of Science, June, 1872.

occur on some of the islands along the north shore, being all that remains above water of the soft formation out of which the bed of the lake was hollowed. But while they yielded to the destructive agency of water, the harder beds of the copper-bearing groups have withstood them, and these, therefore, as on the point of the Keweenah promontory, rise abruptly out of the lake, which has washed away entirely the sandstone from their flanks, or, as towards the base of the promontory, spring at as abrupt an angle out of the horizontal sandstone strata, or else from islands, isolated or in groups, which, however, always bear a marked relation to one another and to the lines of upheaval distinguishable on the mainland.

The lower group of these rocks which we have described as confining the western end of the lake has not produced copper in workable quantities, and differs also in lithological character from the upper group. On the south side of the lake this upper group forms distinct ranges, the more easterly of which are traceable with remarkable parallelism from the base to the point of the Keweenah promontory; and they reappear on the north shore, the more westerly in Isle Royale, in the Thunder Bay and Neepigon promontories, and in the St. Ignace group of islands, the more easterly in Michipicotin and in some of the headlands of the coast.

The age of these rocks is the subject of some difference of opinion. They seem, from the observation of Brookes and Pompelly in Northern Michigan, to conform in strike and dip with the Huronian schists, both uniformly dipping to the north at angles of 50°—70°, and where the Huronian come in contact with the sandstones above mentioned, there is the same sudden alteration in dip as between these same sandstones and the copper-bearing rocks on the Keweenah promontory. Hence, one would infer that the traps and conglomerates of the upper bearing series come next in age to the underlying Huronian schists, and that subsequently to their upheaval were deposited the sandstones whose horizontality has not been broken by any disturbing force. The sandstones are generally attributed to the lower Silurian system.

Copper exploration on the Keweenah promontory have been made at innumerable spots over a distance of 100 miles along the strike of the beds, between the Point and Lake Agogibic, but the mines which have proved productive are confined to three districts, viz., Keweenah Point or Eagle River, Portage Lake, and Ontonagon.

On the Point, the copper-bearing rocks attain their

greatest lateral development, and beds of conglomerate, melephyre, and compact sandstone, with the same dip and strike, stretch from shore to shore. Thence, as they curve round in a south-westerly direction, the range diminishes in width. Some of the first mines opened were on the west coast of the promontory, where, for nearly 30 miles, members of the copper-bearing series form the shore wall.

Here the most productive group of mines is on a system of fissure veins, which cut the rocks of the northern range at right angles to their strike. The Cliff Mine, the first of the lake mines to pay a dividend, and which, from first to last, has distributed nearly 2,280,000 dols. among its shareholders, is on a vein which, though not generally wide, was often filled with mass copper. The copper was associated with quartz, calc spar, and other vein stones. The contents of the fissure exhibited a banded structure, and was influenced markedly by the country rock. In this district, likewise, copper was mined from a bed of amygdaloidal trap, here known as the *ash bed*, and work was also done on conglomerate beds; but, if we except Copper Falls and the Phenix Mines, the operations on the fissure veins alone have been financially successful.

While the Cliff Mine near the point of the promontory was the first to prove that the native copper is more than a mineralogical curiosity, the Minnesota, near the south-western end of the range in the Ontanogan district, became even more famous from the enormous masses of copper it produced. Here, likewise, the copper occurs in veins which, though running with the strata, are palpably subsequent formations consisting chiefly of quartz, calc spar, and laumonite. The vein stone is different from the enclosing rock, the walls are well-defined and often grooved. In the Minnesota, the masses were not only large, but frequently threw off branches into the enclosing rock, which interfered with their being detached in the usual manner by removing the country rock adjacent. The prosperity of the mine ceased after the extraction of a mass of 90 per cent copper, weighing 525 tons, in 1857. No mines here are flourishing at present, nor does there seem to be a like revival of mining industry to what is taking place in the Keweenah Point district on the *ash bed*, under the infection of the successful development of certain beds near Portage Lake.

Portage Lake and River extend so nearly across the promontory at about 60 miles from its point that a canal less than three miles long suffices to give water communication between the east and west shores. The lake is

an irregularly-shaped body, as much as two miles wide where excavated out of the low-lying sandstone, but tapering rapidly where the high, bluff cliffs of the trap beds of the copper-bearing series confine it. While still in the low-lying horizontal sandstone, it throws off towards the north-east a long arm, which expands into Torch Lake, a considerable body of water whose north-west shore almost defines the line of contact between the horizontal sandstone and the steeply-tilted copper-bearing rocks.

As the steamer enters the narrows, and there come into view the towns of Hancock and Houghton facing one another on the opposite banks, the large mills on the lake shore, and the mine buildings and tramways on the heights above, the contrast between the modes of activity and the aims of civilised man, and of the Indians, with whom the traveller, if he has been long on the lake, must have come into close contact, strikes the mind very forcibly.

Where the copper-bearing rocks are exposed by the deep fissures, whose bottom is occupied by Portage Lake, the width of the range is seven miles, and the beds dip at an angle of 54° to the North West. They consist of traps of varying degrees of compactness and shades of colours, interstratified with conglomerates and sandstones.

According to Macfarlane, "the constituent of the traps of the Portage Lake District are principally felspar of the labradorite species, and chlorite of a species allied to delessite, with which are found occasionally mica, small quantities of magnetite, and perhaps of augite and hornblende."* He considers the characteristic trap of the region to consist of:—

Delessite	46·36
Labradorite	47·43
Pyroxene	5·26
Magnetite	0·45
	100·00

The mines in the immediate vicinity of the Lake are on the amygdaloidal trap. Many have been opened both on the north and south shores, but those only on the Pewabic lode—the Quincy, Pewabic, and Franklin mines—have returned profit to their shareholders. Of these three, the best worked, and therefore most successful, is and has been the Quincy, and we shall therefore describe it as being a typical, though the best example, of its class.

* Geological Survey of Canada, 1866, page 158.

It was opened in 1849, and has been worked uninterruptedly ever since, stemming the tide of low prices when almost every other mine was carried down the current.

The lowest level is at 1330 feet along the dip of the bed, and therefore on the incline of the shaft from surface, and the longest level is 1600 feet. The shafts and all the workings are opened in productive ground, where that can be followed; but as the walls of the copper-bearing bed are never well defined, and as tracts of rich ground abruptly alternate with stretches of barren rock, there is found considerable difficulty in keeping to the lode, as it is called. Moreover, from being pinched and poor, or even barren, it will suddenly bulge to 20 or 30 feet of rich rock. The hanging *wall* is composed of a fine-grained, compact, bluish trap, but the characteristic trap beneath is coarse-grained and amygdaloidal, and approaches in appearance to the copper-bearing rock.

The copper bed, however, while likewise generally permeated with small amygdules, is of a deeper red and breaks with a more uneven fracture. The minerals which fill the amygdules in the barren bed, viz. quartz, calcspar, lawmonite, prehnite, not only fill the amygdules here, but likewise form irregular veinlets rich in copper; and the chlorite constituents of the rock prevail so largely in parts as to give it a deep green shade. Pellicles of native copper enveloped in chlorite often occupy the centre of the amygdules. We see here the tendency of the copper to aggregate with the quartz, and the *same* zeolitic minerals as compose the fissure veins of the Eagle River, and the bedded veins of the Ontonagon districts; and, therefore, if we attribute the formation of the one to aqueous agencies, are led to ask whether the same agencies have not had more to do with the formation of the beds and their mineral contents than has generally been attributed to them.

Sheets of native copper occur between the joints of the trap in the copper bed, and formed evidently through infiltration, are found also between the trap blocks beyond the walls of the bed. An indication of subsequent aqueous action is seen in the streaks of clay which smear to a great depth the faces of the trap blocks. A single cross course, carrying quartz, but no copper, is said to have been met with. The width of the bed of copper-bearing ground is supposed to be about 70 feet; not that in any place 70 feet of productive rock has been found, but when copper has been lost on one wall, as much as 70 feet have been driven through what is supposed to be the same bed, and copper

found in what has been taken for the other wall. More than once, cross cuttings for many fathoms have thus resuscitated parts of the mine where it was feared the copper had given out altogether. The suddenness with which the rock will change and lose its metalliferous character is very remarkable, and affects, naturally, the productiveness of the mine from year to year.

Copper-bearing beds alternate, however, with barren trap for a distance of 500 feet, as determined by a cross cut eastward from the 70 fathoms level of the neighbouring Pewabic Mine. In the report of the agent of that mine, in 1863, he anticipates that the following copper beds would be reached at the distances indicated. The results justified his predictions. From the Pewabic lode, the distances of the adjacent strata were :—

As Anticipated.		As Determined.	
Old Pewabic . . .	148 feet.	Old Pewabic . . .	171 feet.
Green Amygdaloid .	285 ,,	Green Amygdaloid .	275 ,,
Albany and Boston .	382 ,,	Albany and Boston .	380 ,,
Epidote or Mesnard .	465 ,,	Epidote or Mesnard .	448 ,,
Conglomerate . . .	520 ,,	Conglomerate . . .	500 ,,

To the West of the Quincy and Pewabic lode, little mining has been done on the lake shore, the Hancock being the only copper-bearing bed extensively worked.

The heaviest copper lies generally near the foot wall. Throughout the region the metal is classed according to its size as mass, barrel, and stamp work. Mass copper is confined to the other districts; but the Quincy Mine yields a certain quantity of barrel work, or copper pieces of such size that they can be separated from adhering rock without the aid of water dressing. The quantity is, however, small, compared with that which is scattered in particles so small that machinery and mechanical concentration alone can separate them from their matrix. The means used to effect the separation are the same in all the mills of the district.

The equipment of the Quincy Mine above and below ground is excellent. The hoisting cars are of heavy boiler plate. Here and at other mines the cars discharge themselves by means of a very simple device. They are shaped like large coal-scuttles, and run on four wheels; but on the same axle, and projecting beyond the back wheels, are wheels of smaller diameter, which, when the car reaches the spot where it is to be emptied, run up inclines secured on each side beyond the track. Thus the back wheels are lifted off the track, while the four wheels remain on the rails and the body of the waggon, tilted forward, shoots out its contents.

Heretofore it has been the custom in the Portage Lake

shore mines to calcine the rock, and thus render it more friable; but following the example of the Calumet Mine, a hammer like a pile driver has been introduced into the Quincy ore-house, which reduces the larger blocks to a size suitable to the Blake crusher, and for hand-picking. The ore undergoes the following treatment. Discharged from the hoisting car, it is carried down an incline to the ore-house, which is on the brink of the steep hill overlooking the lake. The ore-house is provided with a hammer, under which, as stated, the largest blocks, weighing often over a ton, are broken. Such blocks require enormous force to shiver them, inasmuch as they are generally permeated with metallic copper in arborescent masses, which so binds the rock together, that even when broken, fresh force has to be used to drag the detached stones asunder. In the ore houses a preliminary hand sorting of the rock takes place before it is further reduced in size by Blake's crushers. Beneath the Blake crushers, other hand pickers are stationed, who separate still more of the barren or almost barren rock; and the ore, reduced in quantity to about two-fifths of what was hoisted out of the mine, is run down the steep inclined tramway to the copper house.

Stamps are used invariably throughout the peninsula for crushing the ore. Cornish rolls have been tried, but without benefit. They become so often clogged with the larger lumps of copper, and, thus kept apart, so much stuff passes through uncrushed, that the quantity of raff was enormous, and the yield of the rolls small. In the Quincy Mill, when running to its full capacity, 70 square shafted stamps, weighing 900 lbs. each, and with a drop of 16 inches, crush 232 short tons of rock, or 3·3 tons per stamp head per diem through screens perforated with $\frac{1}{4}$-inch holes. Two of the batteries are engaged upon the barrel work, which is, by their pounding action, more perfectly freed from rock than it can be in the ore-house, but has, of course, to be removed from the battery-box; and all the battery-boxes have to be cleaned out twice a day. From the batteries the ore passes on to shaking sieves perforated with $\frac{1}{8}$-inch holes, and fine and coarse are further classified before being concentrated by entering with a full stream of water a succession of long triangular troughs which diminish in diameter and depth as size after size is drawn off to its proper hutch. The hutches everywhere used are piston hutches with fixed bottoms; and though in different mills they go under different names, the differences are in reality trifling. Collom's jigs are those most commonly used, and consist of a central piston-

box divided into two compartments, each of which is in communication with an adjacent compartment in which the sieve is fixed and into which the copper that passes through the sieve falls. The pistons of the two hutches are pressed down by a single rock shaft, and each piston is lifted back into position by a spring—a desirable motion—as the down-stroke is thus sharp and rapid, and the up-stroke slower. But the hutch is open to the objection that, as each piston covers only half the corresponding sieve, a wave is propagated from one end of the sieve to the other, which interferes with the regular subsidence of the ore. As the ore is imperfectly sized, some collects in the sieve and is removed from time to time, but most falls into the bottom, whence it is carried away by the flow of water.

The hutches are arranged, with a view to saving labour, in tiers one below the other, so that the scimpings from one flow into a hutch on a tier below, and the concentrate is re-concentrated in like manner. Water being the carrier, no handling, or very little, is required from the time the ore is thrown into the stamps till it is shovelled from the receiving tank as 80 to 90 per cent copper.

One of the most perfect mills on the lake shore is that of the old South Pewabic mine—now the Atlantic. In it, the stuff crushed by Ball's stamps is concentrated by 112 hutches arranged in seven tiers. There, also, the rotating German buddle is found to save the copper from the slimes effectually and cheaply. In the Quincy Mill, the old-fashioned percussion-table takes out the coarse slimes, and tributers re-treat the refuse from the whole mill in a separate building with English buddles. The coarse concentrate generally runs to nearly 90 per cent of copper, the fine, which cannot be separated, without repeated washing, from the iron—which we have seen is a constituent of the trap matrix—sometimes stands as low as 50 per cent; but all the mills aim at delivering an average of 80 per cent to the smelting works.

Side by side with the Quincy Mill is the Pewabic Mill, in which Ball's stamps are used. A comparison of the tailings from the two mills, made by Mr. Macfarlane, is interesting. He found—

Quincy Mill.		Pewabic Mill.	
Scimpings from coarse ragging	0·06 per cent.	From head of run . .	4·93 per cent.
Scimpings from fine ragging	0·73 ,,	,, middle of run .	3·00 ,,
Buddle tailings . . .	0·46 ,,	,, end of run . .	3·13 ,,
		,, heap outside of run-house. .	0·66 ,,
		,, sand bank . .	1·00 ,,

The Ball stamps may influence the result, through the volume of water required for their efficient working, and which, not being separated from the suspended ore, may in some cases flood the hutches.

The annual reports of the Quincy Mining Company are models, presenting the work done and the cost of doing it in clearest detail. From the report for 1872 we summarise the following particulars. During the year, there were stoped 5165 fathoms, and sunk in shafts and winzes 898 feet—say 150 fathoms, and driven 1974 feet—say 329 fathoms. Assuming the specific gravity of the rock to be 2·7, and that therefore there are 18 tons to the cubic fathom, there were broken 101,592 tons of rock. As there were 60,828 tons stamped, about 4-10ths of the rock was separated by hand-picking. The mining, raising, and picking cost for the year amounted to 283,487·30 dols., or 2·79 dols. per ton of rock raised, while the milling cost was 64,783·79 dols., or 1·06 dols. per ton of rock stamped. This large amount of rock yielded 2,804,954 lbs. of concentrated mineral, which produced 2,276,308 lbs. of ingot. There was recovered, therefore, only 1·12 per cent of copper from the rock mined, and yet there were divided, as the year's profits on working, 210,090 dols.

In 1872, copper brought an exceptionally good price, selling at $32\frac{1}{3}$ cents per lb., but as a set-off, wages were high, the average wage of miners on contract being 60·62 dols., and the yield of the ground per fathom lower than its wont.

Distributing the cost over the mineral produced, we find that, as 2,804,954 lbs. of mineral—which, without making an allowance for the slight loss in smelting, must have contained 81·1 per cent of copper—were obtained at a cost for mining and concentrating of 461,147·83 dols., each pound cost 16·44 cents; but when the cost of smelting, transport, insurance, and commission was added, each pound of ingot cost 22·93 cents U. S. currency, or, say, 20 cents in gold. Copper has fallen to 25 cents U. S. currency, but as wages have declined proportionately, and the cost of production therefore has been lessened, there is not likely to be a very serious decrease in the profits. Besides distributing this large sum among the shareholders, there were added to construction account,—for permanent improvements likely to lessen the cost of future production,—67,227·65 dols., so that the real profits of the year were 277,318·35 dols., which certainly could have been realised only by good management and by the employment of every possible labour-

saving appliance for the working of an ore yielding but 1·1 per cent of copper.

Another mine even more interesting to the mineralogist, and more startling in its yield, is the Calumet and Hecla. It is situated 13 miles from Portage Lake, in a north-east direction, on a bed of conglomerate, which, however, it is not easy to identify with any of the beds that abut on the lake, as the range widens as it approaches the Point and the beds flatten. While the mineral range at the lake is 7 miles across, at the Calumet and Hecla it is 13 miles wide, and the dip declines from an average angle of 54° to 38°. Copper had been extracted from conglomerate beds before the opening of this mine, but never with good financial results. From the Albany and Boston Mine, where both a conglomerate and an amygdaloidal bed are worked, specimens very similar to the rock since yielded by Calumet were obtained; but the failure of this and other mines led to a distrust in, and a too hasty condemnation of, conglomerate mines. It is to be feared the opposite error may now be run into.

The Calumet Mine was discovered about 13 years ago. An inn, the half-way house between Hancock and Eagle River, stood in the forest near where the mine is now, and was kept by a Cornish man. His pig—so tradition tells—fell into a pit, which proved to be an old Indian working. It was dragged out so be-smeared with green that the owner at once suspected the existence of copper. Since then, two little towns,—Calumet and Red Jacket,—have sprung up, and as great a change has taken place beneath the surface of the soil. Two mines on adjacent locations, though in the same bed, viz., the Calumet and Hecla, are owned and worked by one company. This mine has now reached a depth of 1060 feet on the incline of the bed, or 600 feet vertical, and one of the upper levels is 3000 feet long. Most of the copper comes from a bed of conglomerate, in which a hard red porphyritic pebble is embedded in a cement of the same rock, and of native copper. The pebbles in the rich rock are smaller and more rounded than beyond the rich chimnies. The pebbles composing the conglomerate are seldom themselves cupriferous, though some of them are. I have a large pebble from the conglomerate bed which is identical in appearance with the compact chocolate-coloured rock of the Quincy Mine, and is throughout permeated with a little copper in the same manner as the rock, but for a depth of about two lines from the surface it is ensheathed in fine-grained copper, which, as well as the

copper permeating it, may have penetrated the pebble or been deposited around it,—it is difficult to say which. In the conglomerate also occur boulders of solid copper. Some are said to exhibit a concentric arrangement of the copper, but one I had cut through the centre was homogeneous in structure, but contained, embedded in the copper, a few crystals of quartz and felspar.

Interstratified with the conglomerate are thin bands of copper sandstone, the copper being in fine grains, sometimes deposited pure, at others mixed with epidote and quartz or finely-ground porphyry, the laminæ easily separable from one another. In their midst are sometimes embedded pebbles of copper. Bands of hard compact sandstone, from the disintegration of the same rock as compose the conglomerate, are met with beneath the foot wall, on the hanging wall, or in the bed itself. A specimen in my possession exhibits successive layers firmly compacted, some of conglomerate, others of coarse-grained and others of fine-grained sandstone, with a surface distinctly ripple-marked. The aqueous origin of the bed cannot be doubted, but whether the copper was mechanically or chemically deposited, it is more difficult to decide. The easier explanation of its occurrence is on the hypothesis of a mechanical deposition, but, as militating strongly against it, is the undoubted fact that the conglomerate pebbles very rarely carry copper. The effects of subsequent chemical action are beautifully exhibited in a clay *fluccan* which, from the surface to nearly the lowest level driven, lines in places the foot-wall. In it, embedded in soft clay, derived from the disintegration of the rock, and which harden into a mass that might almost be mistaken for a piece of trap, occur with calc spar, laumonite, and quartz, copper in dendritic masses, distinctly crystallised. Some of the specimens taken from the fluccan undoubtedly exhibit instances of false crystallisation, plainly showing the impress of the crystals amidst which they were formed, but others are as undoubtedly themselves crystallised. *Vugs* also occur lined with crystals of epidote, and calc spar, and spongy copper; and through the bed there passes diagonally what is called a dropper. It is only a few inches wide, but consists of what is locally called brick copper, which is accompanied by crystallised silicated minerals, entangled in which are conglomerate pebbles. It has unmistakable *slickensides*, on which the copper is actually polished.

A bed of amygdaloidal trap overlies the conglomerate, and is in places rich in copper. Some of the amygdules are

completely filled with copper, in others a small nucleus of copper is enveloped in calc spar or epidote, while a coating of red ferruginous-looking earth lines the cell. A trap, similar in appearance, is worked by the South Pewabic Mine on Portage Lake, but there its richness is deceptive, for the copper forms in this shell only around an earthy nucleus.

The long levels of the Calumet and Hecla run through three rich chimnies of conglomerate, the longest about 1300 feet. They dip to the north, and are widening out rather than otherwise in its lower levels. Between these rich streaks, large tracts of which will yield a 20 per cent ore, are others of poorer ground, and others still almost barren, which are left standing. The average width of the productive portions is 13 feet.

There are broken, raised, and concentrated, 740 tons of rock a day. To handle such a large quantity, work has necessarily to be thoroughly systematised both below and above ground, and machinery utilised to the utmost.

Each mine possesses six shafts,—or twelve in all,—eight only of which are connected by levels, and four only used as hauling shafts. The shafts are sunk at 400 feet apart, and levels are driven every 90 feet. Between each two shafts two winzes are sunk, and three stopes 30 feet high are opened on each side of each winze, so that eighteen stopes are worked between each two shafts. Six feet of ground are left standing on each side the shafts, and a heavy arch below each level supports the roof, and gives firm foundation to the road-way. A wall of heavy stulls provided with gates at every 10 feet protect the road-ways, and allow large accumulations to be made in the stopes. Timbering is a heavy item of expense, as the trap which composes the roof is very liable to fall out in pyramidal blocks. The mine-work is done by contract,—stoping by the fathom, drifting and sinking by the foot. The contractor must deliver his rock at the nearest hauling-shaft. The traps are 4 feet apart in the levels, and 4 feet 4 inches in the shafts, as the cars have to be large to receive the heavy blocks which break away in the stopes. The miners are allowed to send to the surface blocks not over 1 ton weight, but the cars are constructed to hold 2 tons.

Drifting is done with great economy, by machine-drilling. Seven Burleigh drills of large size, with 2-inch bits, are steadily at work in each mine, and it is found that with them a drift 10 feet wide can be driven at 8·00 dols. less per foot than a 6-foot drift by hand-labour. This calculation

leaves out, however, the cost of the motor power. In the Quincy Mine, the same drills are being thrown aside as uneconomical,—a discrepancy in result which may be accounted for by the fact that in the Calumet there is a well-defined salvage, whereas in the Quincy the drifts are run through solid rock, and grooves must be scooped out beneath the face of this advancing drift,—an operation not easily performed with a cumbrous drill.

The ore is broken in two ore-houses, each of which is provided with a pile driver to shatter the large masses—a Blake's crusher with 18 by 24 inch opening, and six smaller Blakes, with 8 by 15 inch openings, but no attempt is made at selecting by hand, but all the ore raised passes to the mill.

From the crushers the ore falls into huge hoppers, whence it is discharged as called for into the railway cars. All the appliances, in fact, are on a scale such as we are in the habit of associating with iron mining. A five-mile railroad unites the concentrating works on Torch Lake with the mine, and over it two hundred car-loads of 4-ton capacity each are carried daily.

The mills present no feature of special interest. In one are three of Ball's stamps, and in the other four. Six of these powerful machines are running regularly, and crush up the whole yield of the mines. To each stamp there are assigned 20 jigs.

The stamps are steam-hammers. The slide valve is worked by eccentric gearing, and the piston-rod is inserted into the head of the shaft, which is 9 inches in diameter. The stamp-head is 22 by 14 inches, and weighs 6 cwts. Its upper surface is provided with a bevelled ridge, which slides into a slot in the bottom of the shaft, and is then keyed home. When working on the amygdaloidal trap, Ball's stamp heads, made with white iron and a small percentage of Franklinite and tough pig, last a month. At the Calumet Mills they are worn out in six days, but the renewal involves a stopping of the stamp of only 50 minutes. Each stamp works in a separate stamp-box, which is five-sided, and discharges from three sides through steel plates, perforated with 3-16th inch holes. Each stamp can crush daily 120 tons of this exceedingly hard rock, and is said to consume 25 horse-power; 3000 gallons of water a minute are pumped to the two mills. The great advantages of using the stamp are that so much work can be done with so little machinery and in so contracted a space, and that so little time is occupied in repairs. The Calumet Mills never stop. The Quincy mill is idle for about one month out of twelve.

The scimpings are not clean. They carry from 1·40 per cent. to 1·80 per cent of copper, 0·40 to 0·80 of which is as oxide. Twelve tons of copper, therefore, are thrown away daily.

The Calumet Company publishes no report, but the following figures are, if not quite, very nearly correct. There are 1600 hands employed, 260 contracts are set in the Calumet, and a somewhat greater number in the Hecla. The cost of breaking a fathom of ground varies from 20·00 to 22·00 dols., and it yields 21 tons of rock; the cost of dressing exceeds that at the Quincy mine, standing at 1·17 dols. per ton. In 1872 the mine produced 9717 tons of ingot. The quantity of ore raised daily was about 740 tons, or 266,400 tons per year of 360 days; and, therefore, as it produced 9717 tons of ingot, the ore actually yielded 3·6 per cent of copper. This large amount of work was rewarded by profit in proportion; for there was distributed among the shareholders, in 1872, 2,750,000 dols.; and during that same year large sums were expended in permanent improvements. The result in every respect is unparalleled in the history of copper mining; and all owners of copper mines with no such brilliant promise can only hope that it may not be repeated; for the effect of a very few such mines would be most depressing.

Adjoining the Hecla another mine is being opened by the Osceola Mining Company, which, from surface indications, will be very rich. The Allonez near by is expected to turn out well, and on the Isle Royale attention is again being given to long-neglected conglomerate beds, and the prospect of success is there good also. The Royale, though belonging to Michigan, lies close to the Canadian shore. As already pointed out, the copper formation is largely developed from Michipicoten to Thunder Bay on the main land and on Canadian Islands.

With the experience gained on the south shore, explorations could now be conducted on the north, with better chance of success than heretofore. What little has been done has revealed the existence of deposits that would not have remained unworked had they been situated on the opposite shore.

The following statistics, officially correct, are taken from the annual circulars published by the "Portage Lake Mining Gazette."

The production of all the mines on the promontory for the year ending Nov. 30, 1873, was as follows:—

	Tons.	Pounds.
Calumet and Hecla, for year ending Nov. 30, 1873	11,551	1,938
Quincy, for year ending Nov. 30, 1873	1,680	180
Franklin Pewabic	671	1,673
Houghton	285	—
Schoolcraft	270	1,520
Concord	72	—
Isle Royale	143	1,417
Atlantic, for broken season	464	701
Albany and Boston, broken season . .	50	—
Sumner, for year ending with close of navigation	77	—
Other sources	8	—
Total	15,194	1,429
Production in 1872.	12,612	319
Increase in 1873	2,582	1,110

Keweenah Point District.

	Tons.	Pounds.
Central, for year ending Nov. 30, 1873 .	1,081	1,983
Copper Falls, for year ending with close of navigation	834	927
Phenix	350	—
Cuff	279	1,264
Delaware, for year ending Nov. 18, 1873	55	742
Amygdaloid, broken season	19	303
Other sources	2	184
Total	2,781	1,903
Product in 1872	1,836	894
Increase in 1873	945	1,009

Ontonagon District.

	Tons.	Pounds.		Tons.	Pounds.
Ridge . .	150	113	Knowlton .	39	1,864
National .	131	318	Rockland .	16	460
Minnesota.	103	1,700	Mass . .	6	868
Flint Steel	45	1,356	Adventure .	3	1,238
Bohemian .	40	500	Tremont .	—	700
Total				537	1,117
Product in 1872				725	1,000
Decrease in 1873				187	1,883

Recapitulation.

	Tons.	Pounds.
Portage Lake District.	15,194	1,429
Keweenah Point District	2,781	1,903
Ontonagon District	537	1,117
Grand total for 1873	18,514	4,449
Or about	14,811	tons of ingot.

The Copper Mineral (of about 80 per cent), produced from 1845 to 1874.

	Tons.
1845 to 1854	7,642
1854 to 1858	11,312
1858	4,100
1859	4,200
1860	6,000
1861	7,500
1862	9,962
1863	8,548
1864	8,472
1865	10,791
1866	10,376
1867	11,735
1868	13,049
1869	15,288
1870	16,183
1871	16,071
1872	15,166
1873	18,514
Total	194,909

About 150,575 tons ingots; value about 82,000,000 dols. In 1872 there were distributed in dividends—

	Dollars.
Calumet and Hecla	2,750,000
Quincy	350,000
Pittsburgh and Boston (Cliff)	100,000
Central	80,000
Minnesota	50,000
Franklin	20,000
Pewabic	20,000
National	20,000
Total dividends	3,390,000
Total assessments . . .	190,000
Excess of dividends over assessments,	3,200,000

The same mines have been remunerative from their openings, and have yielded 11,810,000 dols.

The paid-up capital on the same mines amounts to the trifling sum—

	Dollars.
Calumet and Hecla . . .	800,000
Quincy	200,000
Pittsburgh and Boston . .	110,000
Central	100,000
Minnesota	436,000
Franklin	370,000
Pewabic	235,000
National	110,000
	2,361,000
Increase of dividend over assessments,	9,449,000

There is, of course, another side to the picture. Of 111 mining companies formed, only the eight above enumerated and the Copper Falls Company have paid dividends. Many of the companies were organised to work locations where there was no copper at all, and others failed through ignorance and bad management. The total amount levied, as far as can be ascertained, has been 19,296,500 dols.

All the copper produced in the Peninsula is smelted at Hancock on Portage Lake, or at Detroit, branches of the same establishment. Detroit takes the mass copper from the Keweenah and Ontonagon Districts, as the furnaces there are constructed to receive it. The roof of the reverberatories are lifted, and masses of 10 tons lowered on to the bed, when the roof is replaced, luted down, and the fires lighted. In the Hancock establishment only the barrel and stamp work of the Portage District is treated.

The mineral from each mine is smelted apart, and the copper returned in ingots; 18·00 dols. per ton being charged for the first smelting, and 12·00 dols. for every ton of slag and coarse copper re-smelted.

In the Hancock establishment there are seven reverberatories and two cupola furnaces.

The copper is smelted without any flux in the reverberatories, in charges of 16 tons. Eight to ten hours are occupied in running down, two to three hours in poling, and three hours in ladling out. When pressed for time nine charges are smelted a week.

The product is about 78 per cent of the copper as ingot, a rich slag which is returned to the reverberatory, and a

poorer slag which is re-smelted in Mackenzie's blast-furnace with lime as a flux. The valuable product from the cupola is a coarse copper of 85 per cent, which is treated in the same manner as the crude mineral, and a poor slag carrying not over 3-10ths per cent of copper.

One thousand pounds of coal are said to be consumed in the reverberatories to every 2000 lbs. of mineral smelted. Poling is done with birch rods. At Detroit, when poplar could no longer be obtained, oak was substituted without affecting the toughness of the metal.

IV. ON THE MODERN HYPOTHESES OF ATOMIC MATTER AND LUMINIFEROUS ETHER.

By HENRY DEACON.

THE generally received opinion as to the constitution of the material universe can be summarised pretty accurately by saying that all natural objects consist of either atoms, which are solid centres of force or of motion,—or of ether, the medium through which the forces or motions of atoms are transmitted,—or of both atoms and ether.

A molecule is defined as the smallest piece of any substance that can retain all the properties of the substance, and therefore the constitutions of the molecule and of the substance are identical.

The different qualities ascribed to atoms and to ether are best reviewed by parallel comparison, of which the following is an illustration :—

Atoms are inelastic.	Ether is perfectly elastic.
Atoms are indivisible.	Ether is infinitely divisible.
Atoms do not touch, or atomic matter is not continuous in all directions.	Ether is continuous.
Atoms coalescing evolve energy, and in separating absorb energy.	Ether coalesces, and is divided without evolving or absorbing energy.
Atoms gravitate.	Ether does not gravitate.
Atoms possess "mass" and inertia, and in motion momentum.	Ether has neither "mass" nor inertia, and cannot be imbued with momentum.
Atoms originate and transmute or convert motion and force.	Ether is merely a medium, and can only transmit motion or force unchanged.
Atoms, being inelastic, move as a whole instantaneously, *i.e.*, motion passes through or is imparted to the whole of an atom, or continuous atoms, without any lapse of time.	Ether is moved by degrees, and time elapses as motion passes through or is imparted to it.

That these converse qualities are necessary corollaries, one of another, will be evident on a brief examination; and whilst incidentally showing this connection, I would criticise those hypotheses which deny the quality of elasticity to tangible matter apparently possessing it, and which simultaneously create a new and totally different kind of matter to receive it.

Atoms are described as swinging to and fro in this wonderful ether, without loss of motion or of energy, and they consequently either impart no motion to ether, or else motion must be communicable to it without absorption of energy. But ether is a medium that transmits motion, or its necessity and office disappear. If atoms do move ether, and any energy be absorbed, the motion which is the energy possessed by the atoms must decrease. If no energy be absorbed, and yet ether be moved, then either ether movements are arrested without result or energy must be created. An alternative is a possibility, viz., all kinds of motion may be imparted to atoms, but only some kinds of motion to ether.

The question then arises—Is it not easier to make atoms elastic and matter continuous, and so altogether avoid the necessity for ether? And before admitting the necessity for the existence of ether, it will be well to review the known properties of matter, and philosophically safer to imagine their extension in the same direction, even to an indefinitely great degree, than to imagine the existence of matter of an entirely different and unknown kind.

Atomic matter forms an indefinitely small bulk in the universe. We see the sun, moon, and stars, and all the host of heaven, but when we calculate their dimensions and distances, and compare the bulk of these bodies with the bulk of the sphere of ether containing them, and by whose aid we know them, figures fail to convey any idea of the indefinitely small bulk of matter compared with the indefinitely enormous bulk of ether.

Taking the distance of the sun from the nearest fixed star (α in Centaur) as the radius of a sphere of ether, and comparing its bulk with the bulk of our solar system, as being the smallest possible proportion in which atomic matter and ether can exist, it is as 11,000 trillions to 1—a proportion comparable, perhaps, to a needle weighing 1 grain in a bundle of 600,000 million tons of hay, and the true proportion of ether to atomic matter must be indefinitely greater than this. It puts this question in a somewhat different light to reflect that the unique and positive qualities

of the mass of matter in the universe are, by inference, without proof, held to differ in essence from the observed qualities of an indefinitely small fraction of the mass, and that fraction a totally different kind of matter.

It is a disruption of the idea of continuity, and of the unity of creation, to assume that the larger and unknown quantity of matter differs totally from that which is known. The proposition that we judge of the unknown by the known implies that the unknown resembles the known, or it cannot be appreciated; and in this sense scientific method requires that the unknown properties of ether should be proved, for they do not resemble any of the known properties of matter.

The admitted want is a medium for conveying force, or, in other words, motion of all kinds; and if we ascertain in what way force or motion is most probably conveyed in known instances, we shall then know more of the probably necessary qualities of the required medium.

Grove's experiment of a silver gridiron on a daguerreotype plate, placed in the sunlight and connected with a galvanometer, proves that the sun's rays are converted into chemical action on the plate, electricity through the wires, magnetism in the coil, sensible heat in the helix, and motion in the needle, exhausting all the forces in the sun's visible rays, so far as known. These forces must all have some property in common, as they are mutually convertible, and that property is motion.

All natural motion is, or becomes, rhythmical. No instance occurs to me either of natural uniform motion, or of motion in a straight line or in a circle. The path of the planets, as regards the sun, are elliptical, and their motions accelerate and retard; and as regards space, their paths are still more complicated. Hence the medium we require to transmit force must transmit vibratory motion.

Vibratory motion is of two kinds, or may be resolved into two kinds, viz., vibration across the line of progression, like all the vibrations or forces accompanying light and the waves of the sea; and vibratory in the line of progression, to and fro, like the waves of sound.

The action or force called gravitation is a special property of atomic matter; on it all the properties of mechanical "mass" depend.

Many of the concrete arguments as to the conservation of force turn on the "mass" of matter remaining unchanged under all circumstances: relative unalterability of "mass" is the chemist's absolute creed.

Gravitation is called attraction—"The sun attracts the earth." Attraction, as the name of the phenomenon, is free from objection; but if attraction be a quality, it is the same in kind as suction, and suction we know is the name —and not the explanation—of phenomena. That one atom or mass of atoms can resist and push, and so impart motion to another, is comprehensible; but that one atom or mass should at a distance attract or suck another towards it is incomprehensible. The fact of the approach is seen, and is called attraction, but the reasons for it have to be sought.

Prof. Guthrie, in 1869, communicated to the Royal Society some experimental results, which assist the comprehension of attraction. He found that a sounding tuning-fork attracted such things as the smoke of a candle and delicately suspended pieces of paper. Due precautions were taken to show this attraction was the result only of the vibration, and not of electricity or currents of air, &c. A tambourine strongly vibrated, and other vibratory surfaces, give more powerful manifestations.

In 1870 Sir W. Thomson pointed out that these results were in accordance with general mathematical laws, applicable to all elastic fluids whose particles were put in motion by immersed bodies, either vibrating to and fro, or whirling about in any way. In Guthrie's experiments the vibrations were those of sound, which act to and fro in the line of progression.

In the sunbeam there are both visible and invisible rays, but all those rays obey strictly similar laws, as to speed, reflection, refraction, and polarisation, and hence must be motions of the same kind, and, like light, are vibrations across the line of progression.

"The sun attracts the earth," and gravity acts when the sun's light is absent; hence the lines or rays of the force of gravity are independent of the sunbeam, and probably follow different laws.

So far as attraction is concerned, Guthrie has shown that to-and-fro vibratory motion suffices, and, as all known effects of gravity can be shown to follow from attraction, gravity itself may be the effect of to-and-fro vibrations. It is necessary, moreover, that these vibrations be such as the vibrations of light, &c., do not affect.

Mechanically, all motions that are at right angles to each other proceed without what is technically known as "interference," or, so to say, proceed uninterruptedly. A cannon, whose axis is parallel to the horizon, may project balls with very small and with very great velocities, but each ball will

touch the same horizontal and lower plane at the same time, that time being the same as would be occupied by the fall of a ball in a vertical line from the cannon's mouth to the same horizontal plane. To-and-fro vibrations would, in this sense, be unaffected by transverse vibrations which are at right angles, and thus to-and-fro vibrations of gravity would be unaffected by the transverse vibrations of light, &c. Gravitation vibrations, if ultimately passing into space, would cause a body to lose weight.

It is supposed that suns and planets lose heat from the motion they impart to the rays or vibrations of radiant heat, but the effects of the sun's and planets' loss of heat are not now astronomically observable, and the effects of the loss of gravity and of heat may perhaps be observed simultaneously. As, however, gravity and heat rays are not of the same kind, there is probably a difference in the rate of their dissipation, and the force of gravity may be the weakest but most enduring. In the sunbeam there are both light and heat rays whose vibrations are of the same kind. Incandescent bodies yield similar duplex rays, but their radiant light is extinguished before their radiant heat; and we may conclude that if the sun's condition changes it will be dark long before it be cold; and as similar vibrations are found to dissipate energy, at different rates, dissimilar vibrations most probably follow the same rule.

According to the axiom of the conservation of energy, the rule as to its dissipation is, that energy is parted with or absorbed by work done. A feeble force, or one able to perform little work, absorbs only feeble energy to originate; conversely, a powerful force owes its origin to much energy, and the most powerful force may be the first dissipated.

In calling gravity a feeble force, the necessity of measuring a force becomes apparent. Light, heat, electricity, &c., are termed imponderable forces. This phrase, by implication, admits the possibility of ponderable forces. Is oxygen, for example, a ponderable force? An examination of this question will serve to show the sense in which these terms should be used. Assume oxygen to be a ponderable force, and it is at present impossible to refute the assumption by experiment. Andrews compressed oxygen to about the density of water, and it remained then, as ever, invisible and intangible, except by its forces, its ponderability included. Elements unite with oxygen, and fresh substances of different properties are produced. Heat unites with or is absorbed by ice, and water is produced. The allotropic forms of phosphorus and of sulphur, and many other

instances, will occur to every reader's mind where a substance of different properties is produced by the action or absorption of an imponderable force. A ponderable force would only add weight. If ice became heavier in melting we should say matter was added to it, only because it increased in weight. Radiant heat adds volume as well as sensible heat to the body that absorbs it, so that the addition of two qualities to a substance by the action of one force is not rare. Hence, in the attempt to prove that gravity is a feeble force, the difficulties of measuring forces by gravity, and of the conception of forces as apart from matter, must be remembered.

In many respects gravity well deserves to be called a feeble force. Our atmosphere sustains a load of 15 lbs. to the square inch; we call it a great load,—*i. e.*, a great result of gravity, —but do not equally appreciate the effort of the air in resisting it; it requires an intellectual effort to say the two are equal, and then the magnitude of this effort of gravity does not seem comparatively large.

Prof. Osborne Reynolds has recently shown that a small glass tube, sufficiently strong to be safely used as a gun, with gunpowder, so as to propel little brass rods through a half-inch board, was shattered into the minutest dust when partly filled with water and a charge of electricity discharged through it. A similar result, if obtained by gravity, would have required the pressure of a weight to be measured by many tons on the square inch. This experiment was made to illustrate the effect of lightning upon splitting trees, shattering stones, &c., and Faraday showed some years ago that more electricity was concerned in a dew-drop than ever was manifested in a thunderstorm. Many trees and stones are often shivered to fragments by one storm, showing that an equivalent to enormous mechanical or gravitation forces is latent in a part of one of the forces that builds up a drop of water.

Heat expands bodies, and requires an enormous mechanical or gravitation force to resist it. The rays of the sunbeam, if converted into mechanical force, would far exceed anything we can realise; and heat may be said to act paradoxically. Its absorption develops mechanical force, and so does its subtraction from freezing water and from solidifying bismuth.

The sunbeam also contains actinic or chemical rays, probably the most energetic of all, and for the mechanical equivalent of chemical energy we may again go to Faraday for an illustration.

The metal potassium, like other metals, is mechanically compressible only to a very limited extent: probably no means are known by which so great a weight or pressure could be applied as to compress, say, 450 units of it into the bulk originally filled by 400 units. Faraday points out that a space originally filled by 430 units of the pure metal potassium, if filled by the same potassium when by chemical action made into carbonate of potash, would then contain 686 instead of 430 units of the same metal, and, in addition, 2744 units of oxygen and carbon. He shows that this power of compression is not restricted to carbonate of potash, nor to potassium, but is even more strikingly exemplified when some other substances enter into chemical combination. No other force measured by gravity gives parallel manifestations.

Comparing the mechanical equivalent of gravity with the same equivalent of other forces, gravity is thus proved to be feeble, and, requiring little effort to originate it; it yields small effects, and is the more likely to be very slowly dissipated.

Perhaps an objection may be made that, although astronomically we see no effects of the loss of heat in the sun and planets, yet we see a hot body loses heat on the earth, but do not see it loses any weight whatever.

The hot body is noticeable because it is hotter than its surroundings. It only loses the excess of heat it had, and becomes cooler than the earth only when its surroundings are exceptional, and are, so to say, means through which its heat energy escapes, and it remains of the same heat as the earth, when subjected to the same conditions. Heat is a mode of motion; communicated heat is communicated motion; by substituting gravity for heat we can say gravity is a mode of motion, and communicated gravity is communicated motion. All substances are influenced by the motion of heat: analogically, all substances are influenced by the motion of gravity. The earth's heat maintains the heat in its parts, and the action is reciprocal; so, too, the gravity of the earth and its parts are reciprocal, and similarly of all related gravitating bodies, and hence no loss of gravity is apparent.

Mr. Crookes's recent experiments on the weight of bodies *in vacuo* prove that heat and gravity have *some* connection, and motions at right angles to each other do influence one another, although each passes on distinctly. One of the cannon-balls before alluded to moves in a path which is neither wholly vertical nor horizontal, although the vertical and horizontal planes it passes through are those through which each force alone would have sent it in the same order

in the same times. Vibrations at right angles may therefore have some connection, as on the principle of the well-known parallelogram of forces.

All radiating vibrations which absorb energy in their beginning, and evolve it when they are arrested, necessarily follow the same law, of effects lessening in proportion to the square of the distance from the centre of radiation. The effects of gravity are therefore consistent with its being a force due to radiant vibration.

Before quitting this point of the question, the velocities at which vibrations are known to be transmitted through matter require some notice. The ordinary surface-wave on water (a vibration across the line of progression) moves with a velocity of about 1 foot per second. The waves of light, vibrations of the same kind, pass through water with a velocity probably as correctly represented by 200,000 miles a second. And electricity passes through some metals still more rapidly. The waves of sound (to-and-fro vibrations in the line of progression) pass through water with a velocity of about 4000 feet per second. As there are certainly two varieties of transverse vibrations, there may be at least two varieties of to-and-fro vibrations, and, if so, the velocities of the varieties of each kind may bear the same or similar proportion to each other, and the velocity of transmission of the vibrations of gravity may probably be at least 4000 times as rapid as those of light.

The medium between atoms has to serve for the phenomena belonging to latent or potential energy, or the energy of position. This energy is of two kinds: one as when a weight is raised; the other kind such as a coiled or bent spring possesses.

The sunbeam reaches us, say, eight minutes after it left the sun. The waves of light present to us at any instant are so disconnected with others before and after them that both or either might be arrested without our immediate knowledge. In fact, as the earth sweeps on its course it cuts into fresh lines of waves, and leaves broken lines to continue their course. Each spot on the earth receives part only of one of the lines of waves of light reaching from the sun, and a still smaller proportion of the lines of those waves whose sources are the sun's we call fixed stars. A floating straw is moved on a pond's surface by the waves proceeding from a stone, which fell at a distance, some time before; and, although the spot where the stone fell is still, the straw moves.

All radiated vibrations follow the same rule, and "energy

of position" is due to the fact that the body, in moving, will receive, from the then existent vibrations, an energy the exact equivalent of the vibrations arrested, and probably, but not necessarily, the equivalent of the energy absorbed in placing it in position.

That energy of position is really due to the circumstances surrounding the position, from time to time, is perhaps still more evident from considering the difference in weight of the same body at the earth's poles and equator, 194 lbs. in the former position weighing only 193 in the latter one. But one of the clearest proofs is that of an electro-magnet and an iron bar. For convenience we may imagine the magnet placed vertically, and the iron bar upon it. The bar, being raised, acquires an energy of position due both to the magnetic attraction and force of gravity. It absorbs energy to raise it, and evolves its equivalent in its descent. But it may be raised whilst the electric current is stopped, and fall whilst it is passing. It would then evolve more energy in falling than was employed in raising it. *Vice versâ* if raised whilst the current passes, and falling when it is stopped; and, evidently too, variations in the electric current would vary either the energy required to raise the bar to any given position, or the energy of position after the bar is raised. Briefly, then, energy of position of this kind is in every case dependent on the forces or motion of the present, and is disconnected from the forces, or motion, or energy of the past; in its nature it is variable and accidental.

But latent or potential energy of another kind may and probably does exist, viz., the latent energy of a bent spring; but this raises the question of elasticity as a quality of tangible matter, and it is difficult or impossible to comprehend the possibility of this form of energy, except the quality of elasticity be assumed. A compressed gas on the atomic hypothesis is elastic because the paths of its atoms are shortened, and their impacts more frequent; it is not a quality of the gas, but of its mechanical structure, and is dependent on the motion of its particles. But on appealing to experimental results, we find when any tangible substance is broken up the transmission of vibration is interrupted. Beginning with a cracked bell, we may pass to Tyndall's recent proof that alternate layers of gas of different densities retard sound, and twenty-five alternate layers of coal gas and carbonic acid gas, forming a column about four feet thick, he shows are impervious to sound. The same gases diffused through each other are pervious to sound. Similar kind of phenomena occur in similar

circumstances with other vibratory motions, as light and radiant heat. Hence, two gases diffused through each other possess a quality which is not the average of the same gases alternated in small bulks, but which appears to be of a distinct kind. No evident solution of this problem occurs on the atomic theory. If ether permeate all bodies, and be the medium of transmission, it is both a conductor and non-conductor of electricity, both opaque and transparent to light, both diathermous, and a heat-absorbent; but as this is an evident absurdity, the utmost transparency of solids and fluids to light, radiant heat, electricity, sound, &c., is due to unbroken atomic continuity in at least one direction. But the metal potassium before alluded to is a conductor of electricity, and, therefore, its substance is continuous. Faraday, however, showed that 680 parts of it together with 2744 units of other substances could be compressed into less space than was originally filled with 430 parts of potassium itself; and it is difficult to avoid the conclusion that potassium is elastic; and, if potassium, then all other bodies—solid, fluid, and gaseous.

It is difficult to trace mentally the act of elastic disturbance within the substance of a body, but this difficulty is equally great with ether, and not more difficult with tangible than with intangible matter, and it is more in accordance with scientific method to connect a known or evident property with a known substance which exhibits similar qualities, than with an unknown substance whose mere existence is hypothetical.

Attraction of cohesion, one of the most evident properties of atomic matter, is intimately related to elasticity; but the atomic theory does not explain how. As the present object is rather to criticise an existing theory than to construct a new one, it is unnecessary to account for what the atomic theory leaves obscure; but it is somewhat probable that the true explanation lies in a direction we proceed very incompletely to indicate: mechanically, motions that coincide coalesce without jarring or "interference;" motions that do not coincide jar and tend to separate.

Another experiment of Tyndall's exemplifies these remarks. Holding a glass tube of about six feet long, and two inches diameter by its middle, he rubbed one end of it with a wet cloth and caused it to emit the sound of a musical note from the longitudinal vibrations thus excited. The free end of the tube was shaken into pieces; each piece a ring, and many of them with cracks quite round the tube, but not completely through its substance. It would require a

weight of about 100 tons to produce the same effect by dragging the glass tube asunder against its attraction of cohesion.

The vibrations, due entirely to the elasticity of the glass, passed to and fro the length of the tube, and were reflected from the ends and from any intermediate "nodes." A series of reflected vibrations thus met a direct and distinct, and not coincident, series of vibrations; and this jarring or interference of vibrations, themselves dependent upon the elasticity of the glass, sufficed to overcome, simultaneously, the attraction of cohesion of its substance at many different points of its length, thus showing that cohesion and elasticity are intimately related, and that the form of the motion governs much of its effects. This latter fact is also exemplified in many other ways. Amongst these are Abel's experiments on exploding gun-cotton by detonating powder. A small portion of common fulminating powder produces a most powerful explosion of the cotton, under conditions where a much larger quantity of the more violent explosive, chloride of nitrogen, simply disperses the cotton, and will drive some of its unaltered fibres into oak, &c., without producing any remarkable explosion of any part of it.

Amongst other instances are Professor Reynolds's bursting of the glass tube by an electric spark, and the difference of fracture in a pane of glass by a slow moving stone and by a rifle bullet.

The effect of a rapid blow on water in an open vessel is another illustration. A "Prince Rupert's drop," broken under the surface of water in an open phial, will break a phial that is unharmed if water is absent.

A fact connected with the spheroidal state of liquids is also significant. A quantity of water on a red-hot solid in the well-known spheroidal state is exploded if struck a smart blow, or if the water be dropped upon the heated surface from a sufficient height an explosion ensues. Recently, at an alkali works in Newcastle, serious damage followed from this cause—the instrument being a falling hot "black-ash ball." A still more serious case is on record of a copper foundry being destroyed from a workman spitting into a large quantity of melted copper.

One remarkable characteristic of molecules has been pointed out by Sir John Herschel, who says, in effect, that the exact equality of each molecule to all others of the same kind gives it the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent, and this thought well deserves careful examination.

There appears no difficulty in appreciating the very precise equality of two distinct but otherwise identical musical notes. We can understand that two bodies may make the same number of vibrations in a second, and that two sounds may be identical, excepting in order of time. We can understand that the vibrations of light and heat of two distinct lines of rays may be of the same length, amplitude, and frequency, and so give the same appreciable results of light, colour, temperature, and mechanical force.

When, however, we examine natural objects, we never find two alike; there is always an observable difference. In this sense the multitude of individual facts is overwhelming; and two natural objects, whose only difference is that they occupy different portions of space, or occur at different times, are unknown. There are differences between two tuning forks, each sounding the same note. We must conclude that, as each fork changes with use and time, so it has changed between two soundings, although its sounds may remain unaltered.

Different but otherwise identical vibrations of sound and light have been compared, and have always been found to be the same; whilst all attempts to prove or to obtain perfect resemblance between any two solid, or, so to say, natural, objects, have been failures. Hence, identity of all active properties is more probably due to identity of motions than to identity of quantities or forms of the substances.

Adding to these conclusions the almost axiomatic idea that all force is motion, the enquiry, as to the necessity for an elastic ether, is restricted to the capacity of tangible or atomic matter for conveying vibrations of all kinds.

It is unnecessary for this purpose to discuss the question as to the definite size of a molecule, defining that term as the smallest portion of a substance that will produce the phenomena, whereby the individuality of the substance is recognised. There is no *à priori* reason why that molecule should not be of a definite size in comparison with the definite number and kind of vibrations it has to originate or transmit, and by which alone its characteristics are recognised.

Sound is not produced by one movement, but by a series of movements, and one movement alone is inaudible. Similarly it is probably true that all observable phenomena of this kind are due to a not indefinite succession of vibrations. Mentally, we can isolate each vibration, but practically no single vibration can produce a manifest phenomenon, such as we call sound, light, heat, or the like; and as one of the results of all chemical action is measured

by weight, the force called gravity must bear its part, and, hence, molecules may also be of a definite weight. To this extent there appear to be no analogical or inferential grounds for objecting to this definition of molecules.

The theoretic formulæ for the velocity of the propagation of sound show the velocities to be the same for each gas whatever be the pressure supported by it; and Regnault, by experiments on the large scale with pressures varying in the proportion of 1 to 5, has verified this law. This law is of general application to all vibrations in elastic media; hence, if light be propagated with an ascertained velocity in air, at the earth's surface, it should pass with the same velocity when air is indefinitely attenuated. Astronomers do find that light passes through our atmosphere, and through stellar space with the same velocity; and so far, therefore, as the sun beam is concerned, this space may be filled with attenuated air, instead of the hypothetic ether, if the air be elastic.

The preceding remarks have incidentally illustrated the axiom that all force is motion, and different forces (gravity included) are different modes of motion; but passive existence, and consequently passive qualities, ready to yield more or less to motion of some kind, appear also to be necessary and axiomatic attributes of matter. We cannot make lace without mechanism. The machine possesses various passive qualities; amongst them is the capacity under certain conditions of making lace by the expenditure of energy in the form of mechanical, as distinguished from molecular, motion. Various machines differ in their capacity of transforming an identical energy, and producing from it various results. Whilst different chemical compounds may be compared to different machines, all constructed out of a few materials, and still fewer mechanical elements variously grouped, all the different natural forces, *i.e.* vibrations of all kinds and forms, must be conveyed by one and the same quality, and that most probably a passive quality, common to all matter.

Continuous atomic matter appears to possess this quality to perfection. The waves of the sea are in number complicated beyond computation, and cross each in all possible directions. They descend below the surface, and in a definite zone we perceive the substance of the sea must be in intestinal motion correlated to the visible motion of its surface. The waves of a lake of gas (and those of a large mass of carbonic acid gas, just rendered visible by suspended carbonate of ammonia, are mentally reproduced as these lines are written) are at least as

interesting, philosophically, as the waves of the sea, and enable us more easily to understand how multitudinous vibrations may coexist in an elastic body which as a whole is stationary.

Sir Charles Wheatstone's experiment helps to carry this same idea much further. The tones of a tune played on a piano are conveyed through a wooden rod, and rendered audible in a distant room. Various fundamental notes of different intensities and in different sequence as to time, accompanied by their overtones or clang, and all the mechanical noises due to the mechanism of the piano, &c., pass through a wooden rod, thinned at its end to stand without interruption between two strings, and so rest on the sounding board.

The wooden rod may be replaced by a glass rod, or by a metal rod, or by a column of fluid. That air conveys these sounds is our daily experience.

At the same time that sonorous vibrations pass, we may pass heat, light, electricity, or magnetism, and if a suitable fluid medium be employed, mechanical waves and motion and chemical action may also simultaneously proceed.

Through the Atlantic cable two messages can pass at the same time from different directions. We talk across each other, and at the same instant hear many sounds from different directions; and an indefinite number of people can together see numberless rays of light reflected from the same point on a reflecting surface, and countless rainbows refracted in one rain drop.

From a seed may grow a tree, and from the tree a forest. The germinating cell or group of cells in the seed possess motion of such a form that its communication is the cause of this development; and the development from this cause occurs in, and is communicated by, continuous atomic matter. In continuance and complicity, the communication of motion constituting vegetable and animal vitalism or growth has no known parallel. Tangible matter, therefore, does transmit forms of motion of all kinds, and appears to be elastic; and if elastic, needs no medium for transmitting motion, and the so-called necessity for the hypothesis of ether disappears.

The conclusion, then, must be, it is more philosophical to endow appreciable matter, even hypothetically, with the qualities it appears to possess, than to create matter of an unknown kind in order to endow it with qualities we see, but refuse to appreciate, in matter that lies before us.

V. EXHIBITION OF APPLIANCES FOR THE PRODUCTION AND ECONOMICAL USE OF FUEL,

IN CONNECTION WITH THE

SOCIETY FOR THE PROMOTION OF SCIENTIFIC INDUSTRY, MANCHESTER.

THE chief object of the promoters of this Exhibition has been to concentrate the attention both of producers and consumers of fuel upon the great question of economy, and through the medium of the Society to bring together those who are concerned in the speedy solution of the problem.

The following was the original classification to which the council asked the attention of the exhibitors. No exhibitors have been named in classes 6 and 7; and 3 and 4 it has been found convenient to amalgamate.

(1). Appliances which may be adapted to existing steam furnaces, &c., whereby an improved combustion of the fuel is secured, and a direct diminution in the quantity required is effected.

(2). Appliances which may be adapted to existing steam boilers, &c., whereby the waste heat of the flue gases or of exhaust steam is utilised.

(3). Appliances which may be adapted to existing steam boilers, pipes, and engines, whereby loss of heat from radiation and conduction is prevented.

(4). Appliances which may be adapted to existing steam boilers and engines, enabling them with safety to realise the great economy resulting from the use of high pressure steam or superheated steam.

(5). New or improved furnaces (using solid, liquid, or gaseous fuel), boilers and engines of all descriptions, specially adapted for the saving of fuel.

(6). Apparatus which, by producing a cheap and abundant gaseous fuel, will supersede the costly carriage of coal, obviate the present enormous waste attending its use in the solid form, and condense and save the valuable sulphur, ammonia, and other by-products of the distillation now injuriously affecting iron and other smelting processes, and in a vast number of operations discharged as poisons into the air.

(7). Apparatus or engines for obtaining power advantageously from heat through any other medium than steam.

(8). Natural and artificial fuels of all kinds.
(9). Coal-cutting machines. Peat-manufacturing machines.
(10). Domestic and other fires, stoves, ranges, and apparatus of all kinds (using coal, gas, or other fuel) for cooking, and for warming rooms and buildings.
(11). Mechanical or other arrangements for securing the delivery of proved weights of fuel to the domestic consumer.

Entering at the south door is seen a wooden model of Davey's Patent Differential Expansive Pumping Engine, 200-horse power, for the New Hartley coal-pit; the engine is intended to lift 1500 gallons of water per minute 420 feet high. This is exhibited by Hathorn, Davis, and Co., of Leeds.

On the right-hand side of the building the first object which engages our attention is Erskine's Patent Economiser. This consists of 10 horse-shoe pipes about 4 inches in diameter, and all connected; these are placed in the flue communicating with the chimney. The waste heat from the boiler fire encircles these pipes, and causes the water which flows through them to enter the boiler at a temperature of 280° F.; and as these pipes are liable to become covered with soot and dust, instead of having a scraper, as in many instances is done, a pipe about 2 inches in diameter passes through the entire length of the horse-shoes, which is perforated with holes about 6 inches apart. The pipes are allowed to get hot, and the steam is now blown on to them, which, according to the inventor's statement, effectually cleanses them. The advantages which Mr. Erskine claims are, that from the peculiar form of his economiser, it causes no diminution or obstruction to the draught in the flue. It maintains a thorough circulation of the water through all the tubes, thus preventing the accumulation of scale; it is easy of access to every part, so that if one of the pipes is injured it can easily be replaced.

Andrew Bell shows a fine set of spiral economisers; they have the exact shape of three condensing worms put together. Each worm consists of 70 feet of pipe, and a three-coil machine is sufficient for a 40-horse boiler. Mr. Bell has shown great ingenuity in the casting of these iron worms; it would not be an easy undertaking to cast the worms in one piece—in fact practically impossible. The spirals are cast in half circles, having a spigot and facit joint. The joints fit so well into each other, that the circle can be formed and lifted without the joints parting; moulding boxes are put round these joints, and hot metal run upon them, so that it forms a perfect spiral when they are all

connected. This arrangement does away with all flange joints, so that no leaking can possibly occur, and also secures a perfectly smooth surface for the action of the scraper, which revolves, ascending and descending according to the spiral form of the coil. It is said that a saving of at least one day's consumption of fuel per week is effected. The water having a continuous circulation, all sediment is held in solution and passes through the coils, thereby avoiding deposit. Each coil is tested to a pressure of 300 lbs. per square inch before leaving the works. Economisers of various forms make a great show, and it is difficult to say which is the best.

Messrs. Twibill, of Manchester, exhibit a fine perpendicular economiser, which consists of a collection of tubes set vertically in the flue. These tubes are tested to a pressure of 500 lbs. to the square inch. Some experiments were performed some time since after the heater had been in use for some time. The first test was taken at six o'clock on Monday morning; the temperature of the water in the pipes was 140° F. At four o'clock on the same day it had risen to 284° F., on Friday morning at six o'clock the temperature was 250° F., and at four o'clock the same day it had risen to 310° F.; and the average temperature of the water throughout the week was 273° F. An experiment was performed at Messrs. Romaine and Callender's mill, and the average temperature of the water was 295° F. The scrapers are peculiar to Twibill's machine; they meet round the tubes and have chisel edges, which, by a special arrangement, press against the tube, and actually cut off the soot and tarry matter which accumulates upon the pipes.

Messrs. Green, of Wakefield, exhibit the finest vertical economiser; the joints of their economiser are all turned and bored socket-joints, "metal and metal" forced together by powerful machinery expressly adapted for the purpose. Their economisers are in operation to 65,000 boilers, representing 2,500,000 horse-power.

Nield's improved fuel economiser is on the same principle as Andrew Bell's. This economiser is arranged in sections, each section consisting of a number of cast-iron ring-shaped pipes, through which the water is caused to circulate. The inlet and outlet passages of each ring are close together; and as this is the sole joint, and the only fixed point in the ring, it is quite impossible that the expansion and contraction of the ring can affect the joint in any way; this is a very important advantage, and is peculiar to this economiser.

Robert and Joseph Ellis, of Liverpool, show some ingenious fire-bars, in which the water, before it enters the boiler, is made to traverse these bars, and is raised to a temperature of 300° F. There are many other appliances for heating the water before entering the boiler; there is the Paxman Water-Heater, in which waste steam from the engine is condensed, and so made to heat another supply of water, and the water is pumped into the boiler at a temperature of 200°.

Goodbrand and Holland show a coal-cutting machine. It is a 27-inch self-acting right or left hand coal cutter, constructed specially for the Wharncliffe Silkstone Coal Company to undercut their medium hard coal at bottom of seam.

Messrs. Ommanney and Tatham also have Winstanley's coal-cutting machine. This machine is designed for holing in mines which are worked on the wide work or long wall system. It is driven by compressed air, the pressure required being from 20 to 30 lbs. per square inch, according to the nature of the coal to be cut. The height of the machine is 22 inches, and the gauge of the wheels can be made to suit any ordinary colliery tramway. The cutter holes its own way into the coal, cutting from nothing up to 3 feet or more in depth, the thickness of the groove being 3 inches. The small coal made by holing represents only from 25 to 35 per cent of the quantity of small coal produced by hand holing. The average rate of holing in hard coal, with a pressure of 30 lbs. per square inch, is 25 yards per hour, including stoppages, and this may be considered to equal the work which would be done by at least thirty men in the same time.

Messrs. Hanworth and Horsfall exhibit a self-feeding smoke-burner and fuel-economising furnace. The draw-back to it is the complicated arrangement for effecting the object. The bars are moved by egg-shaped wheels, which gives them a forward and backward motion, and the coal is allowed to fall upon them by means of a sloping plane.

Some experiments were performed at Lacy Brothers, Callis Mill, near Hebden Bridge, upon two of Galloway's New Patent Boilers, 28 feet long, 7½ feet diameter, and working at 90 lbs. pressure, one of them fired by hand, the other by the self-feeding furnace. The results of tests show a gain of 15 per cent in favour of the self-feeder.

Results of Tests, January 22, 23, 1874, at Messrs. Lacy's.

Firing.	Time. in hours.	Coals Used. Cwts.	Lbs.	Water Evaporated. Gallons.	Pounds of Water Evaporated per lb. of Coal.
Hand-fired . .	10·5	75	0	7150	8·42
Self-feeding . .	10·5	69	26	7700	9·93

William Young, Brothers, Queen Street, London, show a Smoke Preventer with spiral bars, for every description of furnaces, grates, and stoves. By means of this apparatus the fuel is introduced at the bottom of the fire, under the burning coals, and thus the production of smoke is prevented. The smoke preventer is composed of spiral bars mounted on an axis, which is moved by hand or machinery each time coals are required; in an ordinary fire-grate, there is a small trough at the bottom, in which works an axis carrying two vanes of fire-bars. The coal is put into the trough, and the poker is used, not to poke the fire, but to turn the fire-bars round, thus turning the fresh coal under the ignited coals.

Messrs. Piercy, of Birmingham, exhibit an apparatus called a Mechanical Stoker, patented by Dillwyn Smith. It is a very ingenious arrangement for feeding the furnace mechanically. In front of the fireplace is a cylinder about 4 feet long and 8 in diameter; up the top of this is a hopper which will hold about 5 cwts. of coal. In the cylinder revolves two Archimedean screws, one right and one left, which carries the coal into chambers, one underneath each screw. In these chambers revolve two fans, which throw the coal the whole length of the furnace, the quantity of coal being regulated by the requirements of the boiler.

Mr. Sudlow, of Oldham, exhibits a Rotary Engine, the advantages of which are as follows:—It obviates the dead points or centres of the crank, and consequent ever-varying leverage, the steam acting at an uniform distance from the centre throughout its entire travel. It also gives an increased longitudinal capacity, wherein to expand high pressure steam without incurring pressure, as in the case of compound engines; also, where necessary, the flywheel can be entirely dispensed with.

Reese and Gledhill show Wright's Patent Movable Fire-Bar. The rapid and complete manner in which they operate upon the combustible matters used decreases the formation of clinker or slag, by removing the refuse while in a state of dust before it has time to cake into a clinker. By their peculiar advancing and retiring action, the slag that is formed at the extreme back of the surface is brought, with every

successive action of the bars, and deposited on the dead-plate or mouth of the furnace. This is an advantage of the greatest importance, as the removal of slag from the extreme back of furnaces has always been attended with great difficulty and the periodical destruction of the fire. A thorough and complete combustion is effected by the breaking up and removal of the slag, and consequent free admission of air between the bars, and a large saving is effected in the usual consumption of coal.

Mr. Gall, of Halifax, shows a Patent Self-Acting Smoke Preventer, with the recommendation that it will reduce the smoke emitted from the chimney by seven-eighths; and should this result not be attained the purchaser may, within one month, return the Preventer to the patentee, and no charge whatever will be made.

Dingley and Son, of Leicester, exhibit Lake's Coal Economiser, which, according to the statement of the patentee, will save from 10 to 15 per cent on stationary boilers, and from 20 to 25 per cent on multitubular boilers. This arrangement consists of a conical, fluted, or corrugated valve, applied to the rear end of the tube, and capable of being adjusted as required; the openings round the valve being the outlet for the draught, and which are proportioned to the area over the bridge, cause the fire to be kept in close contact with the plates around and throughout the entire length of the tube, ensuring more perfect combustion and equal distribution of the heat within the boiler. It is applied without in any way altering the boiler or interfering with present draught.

The Messrs. Howard, of Bedford, show their celebrated Safety Boiler; and among the leading features of this boiler are, safety (every boiler is tested to three times its working pressure, and the bursting pressure of the tubes is at least 1500 lbs. per square inch), great simplicity of parts, facility of repairs, and durability. In the Howard Safety Boiler there are neither seams nor rivets, and no joint is exposed to the direct action of the fire; the tubes are counterparts of each other, and every part is made on the interchangeable principle; and it is more readily accessible, both internally and externally, for thorough inspection and cleaning than almost any other form of boiler—high pressure steam with economy of fuel. With this boiler a pressure of 120 to 140 lbs. per square inch is more secure than ordinary boilers working at 50 lbs., while experience has shown that on the great question of the economy of fuel the hopes entertained that the higher pressure of steam would, under proper

conditions, lead to an important saving of coal have been realised.

A similar boiler is shown, and called the Safe and Sure Boiler, by the Patent Steam Boiler Company, of Birmingham. They claim for their boilers absolute safety from explosion; this advantage is obtained by the subdivision of steam and water in small tubes tested to a pressure of 500 lbs. to the square inch. If a tube should burst, the only result would be a rush of steam and water into the furnace, a sudden lowering of the steam pressure, and the extinction of the fire. A boiler is often thrown aside as useless nine-tenths of which is practically good, but from the remaining tenth having failed the whole has to be rejected; with this boiler that one-tenth could have been replaced by a new one in perhaps less than two hours, when the whole would have been as good as ever. The nature and disposition of the heating surface in this boiler are such as cannot fail to fully utilise the heat applied, while the internal arrangements are such as entirely prevent the escaping gases becoming much above the temperature of the steam, and ensures, with an ordinary amount of attention, perfect consumption of the smoke.

Mr. Stanley, of Sheffield, shows his Patent Furnace for Smelting Ore. The advantages of these furnaces are, in the first place, they effect a saving of from 25 to 30 per cent in fuel. The use and expense of grate-bars are dispensed with, as these furnaces have closed fire-places formed in brickwork; they make from 80 to 90 per cent less ash than open fire-grate furnaces, the workmen have much less labour in working these furnaces, and the heat is quicker and more under the control of the furnace men.

Bailey and Co., of the Albion Works, Salford, make a very fine show of Pyrometers, Hallam's Ejectors, Oil Testers, and other useful inventions. Bailey's Patent Pyrometer is used for indicating heat, saving coal, and promoting uniformity of production in malt-kilns, ovens, and in other places where a certain degree of heat is requisite. The pyrometer for malt kilns is 4 feet long, and has an enamel dial 4 inches in diameter; the dial is indicated at 300°—black figures on a white ground. One of these pyrometers has been tried at the Valley Mill, near Holyhead, and the proprietor has tested it and found it very sensitive at any change of temperature, enabling the man to keep his kiln at the proper heat, which is very important in malt-kilns. These pyrometers are also used by the Government departments for baking bread; it is also used for indicating the waste heat in flues of works and locomotives, for indicating the

temperature of blast-furnaces, gas retorts, and other useful purposes where high temperatures are used.

Bailey's Oil Tester is a very ingenious instrument for finding out the value of oils as lubricants; and if good oil is used for lubricating, it reduces friction in the machinery, and thus saves coal and wear and tear. The tester may be briefly described as a piece of 3-inch shaft and two brass steps, upon which frictional pressure is obtained by weighted levers; a thermometer is fixed upon the machine, to denote the temperature. One drop of oil is put on to a drum of 3 inches diameter, friction is applied, and the "life-time" of the oil (which is the technical term) is indicated by means of a speed indicator, which indicates the number of revolutions required to raise the temperature a given number of degrees. The exact money value of oil may be arrived at as follows:—Suppose a certain quantity of No. 1 oil on the machine shows 200° by being driven 10,000 revolutions, No. 2 oil shows 200° and 7500 revolutions, or 25 per cent less value. In addition to this practical way of obtaining a result, the machine may be driven to a higher temperature, to see which oil produces the worst residue. In testing various oils, a certain weight or measure must be taken; the thermometer should always indicate the same temperature at starting. It is found that 200° F. is the best to try all oils to, if their lubricating power is to be consumed, and the machine should be always driven until that temperature is indicated, and then immediately stopped; the bearings of the spindle should be well oiled, to prevent friction in the wrong place; when a temperature of 200° has been obtained (the speed index showing *zero* at the start), it should then be seen the number of revolutions taken to produce the temperature. After testing the oil, it is directed that the machine be stopped, and the oil is to remain on the machine, and in twelve hours after it is to be tested again to see how soon 200° can be obtained; the second experiment will indicate which oil is the inferior on machinery when stopped. The following is a short table of results on trying these various kinds of oil:—

Quality of Oil.	Temperature produced.	Total indicated Speed of Three Tests.	Market Price per gallon. *s. d.*	Real value of the Oils, taking No. 1 as a standard. *s. d.*
No. 1. . .	200	120,000	6 0	6 0
No. 2. . .	200	180,000	4 0	9 0
No. 3. . .	200	60,000	2 6	3 0

It will be seen that No. 2 oil will allow 50 per cent more

revolutions to be performed than No. 1, and must therefore be worth 50 per cent more money.

Messrs. Johnson and Hobbs, of Manchester, exhibit a model of a Patent Apparatus for the Condensation of Smoke, Gases, &c. This apparatus is exceedingly simple and inexpensive in its construction; it is on the paddle-wheel principle, with an addition of projections on the blades to produce a finely-divided spray of water, which falls through a series of network composed of laths, brushwood, shingle, or other material, and is so arranged as may seem best for arresting the substances to be operated upon. The same liquid may be used over and over again, until charged to any extent that may be desired. The inventors declare that this machine will be found more effective than the expensive condensing towers now used for the purpose, as it produces a powerful draught, which can be regulated at will, and the solution can be made in the machine as concentrated as may be required. The machine has been tried in condensing ammonia, and has been found to succeed thoroughly; the working parts of the apparatus can be arranged to resist the action of hydrochloric and other powerful gases affecting metal work.

Crossley Brothers, of Manchester, exhibit an Atmospheric Gas Engine. This engine works as follows:—Gas and air, mixed in such proportions as to give a mild explosive compound, are admitted under a piston which slides air-tight in a vertical cylinder open at the top. The compound is ignited, explodes, and the explosion drives the piston upwards. The ignited gases, having increased in volume, lose their heat; their pressure becomes less as the piston rises, and when it has got to the top of the cylinder a partial vacuum is formed, and the pressure of the atmosphere makes the piston descend. The work thus done steadily by the atmosphere during the return stroke of the piston yields the driving power, which is transferred to the shaft by suitable mechanism. This utilisation of the instantaneous power of the explosion, by allowing the piston to fly up freely from it without doing other work than emptying the cylinder of air, is the basis of the economy and success of these engines. The sudden energy of an explosion cannot be economically applied to push a piston slowly along against a load, as in the case of steam-engines; it is thus that other gas engines have been superseded by this patent. Some of the advantages of this engine, compared with steam engines, are that it can be started at a moment's notice, and will at once give out its full power; thus no

time is lost in waiting to get up steam. The attendance required is exceedingly small, averaging one hour per day for a man, including cleaning, oiling, stopping, and starting. The fuel has not to be got into the house, nor ashes to be got out; gas is laid on, thus much trouble is saved. No constant supply of water is required; a quart a day suffices. Gas at 4s. per thousand feet will feed the engine at one penny an hour per horse-power. Gas can only be burnt in exact proportion to the power required; this is controlled by a governor. These engines cannot be used for high horse-power; from one to two horse-power is the most they can be used for. From the many testimonials received, it seems that the cost of gas is less than one penny per hour.

The show of fire-grates, kitchen ranges, various kinds of coal savers, is very good, and perhaps the most complete in the Exhibition. The grates, &c., are all in use in the third annexé of the building, so that spectators can judge for themselves as to the relative merits of the various inventions. What would have made this show still more interesting would have been to have given the weight of coal consumed by each fire during the day to produce the desired effect; as it is, one sees an interesting collection of machines for saving fuel, but no experiments seem to have been performed by competent judges to test the truth of each inventor's statements. There are various grates for utilising the waste heat of the fire and causing it to warm air-chambers, which warm air is carried to different rooms in the house.

Shillito and Shorland exhibit patent grates and hot-air boxes for extracting waste heat from every description of grates and kitchen ranges, thereby effecting a saving of at least 50 per cent in fuel, without at all interfering with the general appearance of grates. One of their 30s. boxes can be inserted behind register or sham register stoves now in use, and could also be placed behind a kitchen fire without taking down the range or grate, and, according to the inventor's statement, will raise temperature in excess of external atmosphere from 10° to 20°, and discharge into room or lobby 2000 cubic feet of warm air per hour. The advantages of this fire-box grate over the ordinary grate are, that it secures a supply of perfectly fresh, warm, pure air, and diffuses it equally over the whole room, or rooms requiring to be heated, the cold air admitted from the outside being perfectly fresh, and warmed by passing over the inside back of the grate. The objection to other hot-air stoves, that they draw their supply from the already vitiated

air of the room, is obviated. When this grate is used in dwelling-houses two rooms can be heated by the same fire—the open fire serving for one room, and the heated fresh air being thrown into the next.

Thomas Whitwell exhibits a grate on a similar principle. By his fire-place he injects warm air into the room at a temperature between 65° and 115° F.

Rogerson and Co. show Corbitt's Improved Economic Warming and Ventilating Grate. It is simple in construction. The best points of the modern grate are preserved, viz.—The cheerful open fire; large reflecting and radiating surface; reduced size of fire-box, with convex back, which is composed of fire-brick, and, being a bad conductor, throws the heat into the room; the draught-flue, opening into the chimney, is regulated by Louvre valves, so that no waste heat need pass up the chimney beyond the products of combustion.

The most successful and ingenious fire-grate in the Exhibition is the invention of the Rev. J. Wolstencroft, and is called the Vacuum Draught. The inventor says the great difficulty is solved, viz., "how to get a healthy, cheerful fire, imparting a genial heat, with half the amount of fuel commonly used." We saw the grate in use, and we must candidly admit it was the most cheerful and the brightest fire in the place, but as to the amount of coal it daily consumed we are unable to say. According to some experiments which have been performed with it by James D. Curtis, Commander Royal Navy, there is truth in the inventor's statement, that there is a great economy in the consumption of fuel. Captain Curtis, of Brimpsfield, Gloucester, experimented with the grate in his harness-room from the 18th of August, 1873, to the 1st of September, 1873, using no other fire, burning slack coal delivered for 24s. per ton, employing this fire daily for cooking small things, such as boiling potatoes for the fowls, &c., and after the daily use the fire was left to burn itself out during the night; the cost of coal per day was 3½d. The front of the grate is continued down to the floor, cutting off the supply of air from within the room; by this means an air chamber is formed under the grate, to which the air is communicated from within or without the building, bringing the draught under and directly through the fire-bars. In a fire-grate which has been fitted up in Manchester, at the office of one of the Local Boards, the air-chamber communicates with the main sewer, and draws its supply from thence, thus, as it is supposed, ventilating the sewer,

at the same time consuming the noxious sewer gases. Any kind of fuel can be used, and very small coal can be burnt as easily and with as good results as lumps; coke and cinders may be burnt over and over again, until they become as fine as sand. The ashes from the fire all drop through the bottom of the grate into or through the air chamber, consequently dust from the fire is greatly diminished in the room; the draught may be regulated at pleasure with a valve. The invention may be easily applied to many existing grates at the cost of a few shillings.

By the side of Wolstencroft's fire-place was Kenyon's Patent Coal Saver, which consists of a perforated fire-brick tile, to put into the grate and fill up the coal space, throwing the hot coals to the front of the fire-place, while the back of the fire is comparatively cold. It has the disadvantage of presenting a very dull fire while it is carrying out its principle of saving coal, presenting a great contrast to Wolstencroft's; indeed one might almost think it was placed there as a foil for his more successful competitor.

Crawshaw's Household Coal Saver is a corrugated piece of iron or clay placed behind an ordinary coal-fire. It radiates the heat from the fire into the room, instead of allowing so much waste heat to pass up the chimney.

Frisbie's Patent Feeder and Grate is a most ingenious arrangement for feeding a fire with coal from the bottom. This feeder and grate provides a simple method of feeding fuel up, from underneath the fire, into all descriptions of furnaces, fuel-boxes, and fire-grates. By this principle of feeding from below the fire there is no fresh consumption of the fuel, the igniting of the fresh coal is a gradual process, while at the same time a very intense heat is obtained. The hottest portion of the fire being constantly at the top utilises the heat, and preserves the fire-bars from being burnt out; the heat of the surface of the fire is not abated by the supply of fresh fuel, and no cold air is admitted to the furnace while feeding, thereby preserving a perfectly uniform heat. By feeding from beneath, the coal is pushed up and outwards equally from the centre of the grate, and is evenly consumed, with scarcely any refuse except fine ashes, which drop down through the grate-bars without raking. From various testimonials which the inventor has received, it seems that there is a great saving in the use of the coal; thus one firm says their coal bill averaged £160 a month, but on introducing one of these burners they only used that quantity in four months.

Folloms and Bate, of Manchester, exhibit a large collection of stoves and fire-grates. One of their novelties is a Portable Water-Boiler, which consists of an upper and lower chamber, and is so constructed that the upper chamber is filled with cold water, and as the hot water is drawn off from the lower, the cold water is allowed to fall down through a small pipe, so that there is a constant supply of warm water. It will boil 11 gallons in 20 minutes, or three or four hundred persons can be supplied with hot water for tea at a cost of 3 lbs. of coal.

There is a very good show of various kinds of peat and patent fuel, with the necessary apparatus for condensing and purifying peat.

Kidd's process for carbonising peat consists of a large chamber or drying-room connected with a boiler which supplies superheated steam; from the boiler a steam-pipe passes through the furnace, and from thence into the flue; the steam, in its passage over the boiler-fire, becomes superheated, and, together with the smoke, passes into the drying-chamber; the peat, cut into pieces about the size of bricks, is put into a framework which runs upon wheels, so that it easily runs into the drying chamber, and is run out again when finished, thus saving a great deal of labour. The object of Kidd's process is the collection of the heated gases referred to in a closed chamber, where they may be usefully employed in charring peat, or converting it into charcoal; an artificial draught is created by jets of superheated steam, and the whole products of combustion from the furnace are forced into and retained by the closed chamber. The chamber is filled with peat, which may be dried and charred in less than forty-eight hours by the action of the furnace-gases and superheated steam; the temperature of the chamber soon rises to between 300° and 400° F., and remains at some temperature between those limits. By charring the peat at a low temperature the loss of hydrocarbons is very small, the gases which are poured into the chamber being for the most part non-supporters of combustion; consequently it is impossible for the peat to take fire during the process of charring. The fuel used in the furnace which supplies the gases and generates the steam is peat which has been partially dried in the open air. It is estimated that a ton of peat charcoal can be produced by this method at a cost of 13s. 6d., which sum includes all charges for interest on capital, royalties, and labour; raw peat at 3s. a ton; that used for fuel, 4s. 6d. per ton. Peat thus prepared produces a gas of high illuminating power, ranging between

20 and 22 candles, and 6000 and 9000 feet per ton; the gas is generated so quickly that three charges of peat can be worked off to one of coal, thus effecting considerable economy in the plant of gas works. The charcoal which remains after the gas has been extracted is also much more valuable than the ordinary coal-gas coke. There is, no doubt, a large field open for commercial enterprise in the manufacture of peat charcoal, owing to its freedom from sulphur and its affinity for oxygen at a high temperature. It is equal to ordinary charcoal for refining iron, steel, and other metals. In France, this charcoal, under the name of *carbon roux*, is largely used in the manufacture of gunpowder; it has been used as a fertiliser also for filtering water and town sewage, and when combined with a proper admixture of phosphate of lime it has been found useful as a substitute for animal charcoal.

Henry Clayton and Son show some fine machinery for preparing and forming blocks of condensed peat. One of the difficulties in preparing peat for the market is to get rid of the large amount of water which it contains, as sometimes it is met with containing from 55 to 80 per cent. Of this, a variable proportion is "loose, or free water," much of which, when present in the larger quantity, can be extracted by means of drainage and squeezing; the great bulk, however, of the water is "locked up," confined in the rooty or fibrous portion of the peat. So retentive is peat of this fixed water, that no pressure, however powerful, can effect its expulsion while the peat remains in its natural condition. The objects which the Messrs. Clayton aim at are:—To get rid of as much water as possible by draining and squeezing, then to thoroughly cut up the fibrous or rooty portion, releasing the great quantity of water and air which was previously fast in the fibre, and reducing the whole to an uniform state of pulp. Peat thus prepared will freely and rapidly part with its moisture by natural evaporation, and in so doing will consolidate itself, and thus acquire a density which no pressure of the peat in its natural state could produce, becoming very hard and compact, and of a specific gravity nearly equal to coal; in this state it is (unlike the common prepared turfs) non-absorbent of water. The patentees of the condensed peat say that it produces little or no smoke, contains no sulphur, ignites more readily, and diffuses the heat more generally and more widely than coal itself, leaves no cinder and but little ash. To accomplish these objects with peat direct from the bog, the peat is filled (as dug) into squeezing-trucks, and during its convey-

ance from the bog to the machine much of the "free water" is pressed from the raw peat by a simple and easy means. From the trucks the peat is discharged into the machine, which, in its action, continuously cuts up minutely the fibrous portions of the peat, and produces a perfect admixture of the cut up fibre and rooty matter with the pulpous portion, thereby utilising the whole mass of the bog and entirely destroying its original character and natural spongy nature. In its travel through the machine the material further undergoes a moderate amount of pressure, and acquires a density and form permitting it to be discharged and deposited upon portable trays in blocks or briquettes of convenient size, and thence conveyed by a simple and labour-saving contrivance to the drying-sheds, where, after three weeks' drying (during average weather), the prepared peat becomes hard, compact, marketable fuel. A trial of condensed peat was made some time since for railway engines on the Belfast and Northern Counties Railway, with a view of testing its qualities as a fuel for locomotives. The engineers who made the trial say: "In order carefully to watch the power of the fuel in the generation of steam, we rode on the engine from Carrick Junction to Ballymena, a distance of twenty-seven miles. The pressure at starting was 100 lbs. on the square inch; the commencement of the journey was up an incline of about 1 in 80, 4 miles long, and with double curves. While going up the incline the pressure rose to 110 lbs., and afterwards to 120; the speed, whenever this was permitted, was 40 miles per hour."

Particulars of the above Locomotive Trial of Condensed Peat Fuel.

Total quantity of fuel used	14 cwts. 1 qr. 14 lbs.
Weight of train, including engine and tender	70 tons.
Number of carriages	Seven.
Miles run	74.
Time running	3 hrs. 9 mins.
Weight per mile used of peat fuel .	21·47 lbs.
Average pounds per mile for the last three months, using Welsh and Scotch coals at a ratio of 2 of Welsh to 1 of Scotch	25·25 lbs.
Average for the month of May last .	26·29 lbs.

The engineers conclude their report by saying:—"Having carefully noted all these facts, we have no hesitation in saying that we consider the condensed peat in every way well adapted as fuel for locomotive purposes."

A series of experiments have been made at the Commercial Gas Works, London, on condensed peat, the results of which are given below :—

Yield of One Ton of Coal.

Description of Coal.	Cubic feet of Gas.	Illuminating Power in Sperm Candles.	Cwts. of Coke.	Gas per ton, equal to lbs. of Sperm.	Sperm corresponding to Gas of Boghead Cannel No. 2, equals 100.
Staffordshire	7,100	12·42	13·5	302	13·6
Derbyshire	7,600	11·71	17	305	13·7
Lochgelly	8,000	18·00	13	494	22·2
Derbyshire Cannel . .	8,500	20·60	15	600	27·0
Wigan Cannel . . .	10,000	20·00	13·25	686	30·9
Newcastle Cannel . .	9,800	25·00	13·25	840	37·8
Lesmahago Cannel .	10,500	40·00	10	1440	64·8
Boghead (No. 1) . .	12,500	40·00	8	1713	77·1
„ (No. 2) . .	13,000	48·00	6	2222	100·0
Yield of One Ton of Condensed Peat.					
Belfast	10,500	15·65	8	562	25·3
Creavelea	9,240	18·75	8·75	594	26·7
Welsh	11,000	22·50	7	849	38·2

The following, from a tabulated statement giving details of the various peat enterprises actually now working, will afford some interesting information on peat manufacture :—

System.	Where Working.	Horse-Power.	Tons of Dry Fuel per Machine per Season.	Cost per Ton.	Relative Value of to Coal.
Hodge's Canadian Peat Company.	Montreal, for Grand Trunk Railway.	16	4000	6s. 6d.	5-6ths, or 84 p. ct.
Boston Peat Company.	New England States.	12	4200	8s.	84 p. ct.
Roberts, Pekin.	New York.	13	3500	8s. to 9s.	84 p. ct.
Haspalmoor. Colbermoor.	Bavarian Government Works.	Not stated.	Not stated.	12s.	60 p. ct.
Box's.	On trial.	16	Not stated.	Stated 4s. 8d.	—
Clayton and Son.	Great Britain and Germany.	6	3500	3s. 6d. to 5s.	Variable according to quality of peat.

From the foregoing it will be seen that peat fuel possesses a calorific power of five-sixths of coal, and can be produced in Canada and the United States at from 6s. 6d. to 9s. per ton, where wages for labour are not lower than 7s. per day.

The Peat Coal and Charcoal Company make a show of peat in all its stages, from the time it is taken from the bog to the time it is compressed and carved, for they have some pieces which have been cut into flowers and fruit, till it looks like carved ebony. This Company has bought the patent rights of M. Challeton de Brughat ; his process consists in making peat coal having nearly the same density as pit coal, and also he claims to have invented a better

method of preparing peat charcoal. The cost of this peat coal at the manufactories may be taken at 8s. per ton for small quantities, and 6s. to 6s. 6d. for large quantities. $1\frac{1}{4}$ tons of peat coal made by this process is reckoned to be equal to 1 ton of best English coal; for stowage it will only take, on an average, 20 per cent more room than ordinary coal. This Company intend to establish their first manufactory on the borders of North Wales and Shropshire. The peat on this land is of the best quality, averaging in depth about 12 feet. A trial of this peat was made on the Thames, on board the paddle-steamer *Times*, in the presence of the Duke of Sutherland and a distinguished company. The steamer ran from Beckton gas works to Greenwich in twenty-five minutes with a strong head wind, slack water at top of tide; and the quantity of uncompressed peat fuel consumed in this twenty-five minutes' run was about 210 lbs., maintaining a steady steam pressure of 50 lbs., without smoke, and at all times a good clear fire. The experimenters state that for the generation of steam it requires but a very moderate current of air, is absolutely smokeless, and gets up steam equally quick as coal, and maintains it with a less expenditure of fuel, does not injure the fire-bars, and is in every respect much cleaner than coal or coke.

The South of Scotland Peat Fuel Company exhibit fine samples of peat, which have been analysed and reported upon by Mr. Heddle, Professor of Chemistry in the University of St. Andrews. The composition of the dried fuel, on analysis by combustion, is—

Gas	60·293
Carbon, free	31·064
Ash	8·643

In its ordinary condition, however, it contains—

Water	16·740
Gas	50·200
Carbon	25·864
Ash	7·196

It was found that a sample kept for some days under cover contained 16·4 per cent of moisture, and that samples artificially dried regained upon exposure nearly the above amount, so that it may be held to be impracticable to improve the fuel in this respect; the fuel yields gas at the rate of 7984 cubic feet per ton. When examined by Lewis Thompson's fuel test-apparatus, the calorific power of the fuel was found to be—

In its usual state . . .	4·675
When dried	5·940

That is, one part of the fuel will boil off as steam above $4\frac{1}{2}$ times its own weight of water from 212°, and the dry fuel about 6 times its own weight.

Mr. A. C. Pelly shows his patent peat fuel, which he condenses into solid balls, of the density of hard wood; the peat balls, when manufactured ready for use, cost only about 5s. 6d. to 6s. per ton.

Professor Reynolds reports upon the process, which consists in pulping the raw peat in a horizontal cylinder, within which a shaft carrying a number of arms is made to rotate rapidly, by steam or other power. The fibre is not only broken in this machine, but, owing to a screw-like action of the shaft, the peat pulp is forced through a circular opening, and then appears as a cylinder of pasty material, which is cut into short sections by very simple apparatus; the short cylinders of pulp so obtained fall immediately into a truncated cone, revolving rapidly. Here each piece is made to assume a rough spherical form; these pieces are then dried. The dry product of these simple operations appears in the form of irregular balls; hence the term ball-peat. The following is Professor Reynolds's analysis of two samples of ball-peat:—

Hydrostatic moisture . .	15·12	14·87
Carbon	46·95	47·22
Hydrogen	5·01	5·14
Oxygen	29·83	31·22
Nitrogen	0·38	0·74
Ash	2·71	0·81

Professor Reynolds says:—It is well known that the heating effect practically obtained from ordinary rough turf rarely exceeds 40 per cent of that afforded by Staffordshire coal; this ball-peat possesses a heating value equivalent to 55 per cent of that of the class of coal mentioned, or, in other words, to produce the heating effect obtainable from 1 ton of average Staffordshire coal it is necessary to burn about $2\frac{1}{2}$ tons of ordinary turf, while 1·8 tons of ball-peat would give the same amount of heat.

Reuben and Israel Levy exhibit "Leigh's" Patent Phœnix Fuel, which consists of refuse from coal fires mixed with tar or pitch, and made into balls. We cannot see the economy of the process, as it leaves 75 per cent of ash; they claim the novelty of using the ashes *ad infinitum*.

Radeke's patent artificial fuel consists of small coal, bound together in blocks by the aid of silica, both in solution and in a powdered state.

IV. AN INVESTIGATION OF THE NUMBER OF CONSTITUENTS, ELEMENTS, AND MINORS OF A DETERMINANT.

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THE following paper is an investigation of the number of constituents, elements, and minors, in four classes of determinants, viz., in ordinary, symmetric, skew symmetric, and skew determinants. In calculations and investigations relating to determinants it is often useful to know these numbers independently of the actual formation of the individual constituents, elements, and minors. The investigation of these numbers will be found an interesting exercise in the theory of combinations, and leads to some remarkable symbolical relations.

The references are to Salmon's "Modern Higher Algebra," 2nd edition. The type of a constituent of a determinant of n rows will be written $a^{yz,}$ and the *corresponding* first minor as $A_{yz,}$ (as in Salmon, Art. 2.) All the constituents and all the minors of determinants in the following problems are supposed *finite* and *unequal*, except when expressly stated to be otherwise. Without this limitation the investigations to be given are not necessarily applicable.

I. *Number of Constituents in a Determinant.*

1. The number of constituents in an *ordinary* determinant of n rows is *in general* (*i.e.* if the constituents be all unlike) n^2.

2. In a *symmetric* determinant of n rows, the (n^2-n) constituents not in the leading diagonal occur in pairs, and are equivalent to only $\frac{1}{2}(n^2-n)$ different constituents, making up with the n different constituents in the leading diagonal a total of $\frac{1}{2}(n^2-n)+n=\frac{1}{2}n(n+1)$ different constituents *in general*.

3. In a *skew* determinant* the n constituents of the leading diagonal are in general different, whilst the remaining (n^2-n) constituents occur in pairs equal in magnitude, but of opposite sign, so that there are *in general*—

$$\left.\begin{array}{l} n^2 \text{ different constituents, but only} \\ \frac{1}{2}(n^2-n)+n=\frac{1}{2}n(n+1) \text{ constituents of different } \textit{magnitude}. \end{array}\right\}$$

* SALMON, Art. 37.

† A distinctive name would be convenient for this variety: the author suggests "sub-determinant."

4. An important variety* of the above classes of determinants is that in which *the constituents of the leading diagonal are all zero,* in which case the above numbers are to be all reduced by n: thus the number of constituents will be :—

(1). In an ordinary determinant whose leading constituents vanish, (n^2-n).

(2). In a symmetric determinant whose leading constituents vanish, $\frac{1}{2}n(n-1)$.

(3). In a skew determinant whose leading constituents vanish (this is styled a skew symmetric determinant†), (n^2-n) different constituents, but only $\frac{1}{2}n(n-1)$ of different *magnitude.*

II. *Number of Elements in an Ordinary Determinant.*

Let E_n be the number of elements in an ordinary determinant of n rows.

Then a determinant of n rows may be expressed *in general* as a sum of n different terms, viz., $\Delta = \Sigma_{y=1}^{y=n}(a_{p,y}.\ A_{p,y})$, where $A_{p,y}$ is the first minor of Δ corresponding to $a_{p,y}$, and is therefore itself a determinant of $(n-1)$ rows, and contains (by above notation) E_{n-1} different elements. Moreover, these E_{n-1} elements of *each* term of type $(a_{p,y}.\ A_{p,y})$ in the whole sum, which together make up Δ, are *in general* different from all the elements in every other such term.

Hence, $E_n = n.\ E_{n-1}$.

Similarly, $E_{n-1} = (n-1).\ E_{n-2}$, and so on.

$$\left.\begin{aligned}\text{Hence, } E_n &= n.\ E_{n-1} = n(n-1).\ E_{n-2} = \quad \ldots\ldots \\ &= \frac{\underline{|n}}{\underline{|n-r}}.\ E_{n-r} \\ &= \underline{|n}.\ E_1\end{aligned}\right\}\ (1).$$

$$= \underline{|n} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

since it is obvious that $E_1\ n=1,\ E_1=1$.

III. *Number of Elements in an Ordinary Determinant whose Leading Constituents Vanish.*

$$\Delta,\ \frac{d\Delta}{da_{pp}},\ \frac{d^2\Delta}{da_{pp}.\ da_{qq}},\ \frac{d^3\Delta}{da_{pp}.\ da_{qq}.\ da_{rr}},\ \&c.\ \ldots\ldots$$

represent an ordinary determinant, and its *successive* first, second, third, &c., leading minors, the *leading constituents* (which are of type a_{pp}) being *finite.*

$$[\Delta],\ \left[\frac{d\Delta}{da_{pp}}\right],\ \left[\frac{d^2\Delta}{da_{pp}.\ da_{qq}}\right],\ \left[\frac{d^3\Delta}{da_{pp}.\ da_{qq}.\ da_{rr}}\right],\ \&c.\ \ldots\ldots$$

* See note on preceding page.

† SALMON, Art. 40.

represent the corresponding values of the preceding quantities when the *leading constituents* are all *zero*.
Also let $[E_n]$ be the corresponding value of E_n,

And let ${}^nC_r$ be the number of combinations of n things taken r together, so that—

$$ {}^nC_r = \frac{\lfloor n}{\lfloor r . \lfloor n-r} = {}^nC_{n-r}. $$

It is shown (Salmon, Art. 40) that in general—

$$ \Delta = [\Delta] + \Sigma \left\{ a_{pp} . \left[\frac{d\Delta}{da_{pp}} \right] \right\} + \Sigma \left\{ a_{pp} a_{qq} . \left[\frac{d^2\Delta}{da_{pp} . da_{qq}} \right] \right\} $$
$$ + \Sigma \left\{ a_{pp} a_{qq} a_{rr} . \left[\frac{d^3\Delta}{da_{pp} da_{qq} da_{rr}} \right] \right\} $$
$$ + \ldots + \ldots + (a_{11}.a_{22}.a_{33} \ldots a_{nn}) \ldots (3). $$

Hence, noting that the number of ways in which a continued product, as $(a_{11}.a_{22}a_{33} \ldots . a_{rr})$ of r constituents can be formed from the n leading constituents (of type a_{pp}) is ${}^nC_r$, also that the number of elements in an rth minor of type—

$$ \left[\frac{d^r\Delta}{da_{11}.da_{22}.da_{33} \ldots . da_{rr}} \right], $$

(being a determinant of $(n-r)$ rows whose leading constituents vanish), is by above notation $[E_{n-r}]$, also that by the notation the number of elements in Δ is E_n, and in $[\Delta]$ is $[E_n]$, it results that—

$$ E_n = [E_n] + {}^nC_1 . [E_{n-1}] + {}^nC_2 . [E_{n-2}] + \ldots + . + {}^nC_r . [E_{n-r}] + $$
$$ \ldots . {}^nC_{n-2} [E_2] + {}^nC_{n-1} [E_1] + 1. $$
$$ = [E_n] + \frac{n}{1} . [E_{n-1}] + \frac{n(n-1)}{1.2} . [E_{n-2}] + \ldots + \frac{\lfloor n}{\lfloor r . \lfloor n-r} . [E_{n-r}] + $$
$$ \ldots . + \frac{n(n-1)}{1.2} . [E_2] + \frac{n}{1} . [E_1] + 1. \ldots (4). $$

Now the numerical coefficients are the same as in the expansion of the binomial $(x+1)^n$, and the suffixes of $[E]$ are the same as the indices of x in that expansion; hence modifying the notation with the interpretation—

$$ \left. \begin{array}{c} [E]^p = [E_p], \\ [E]^p . [E]^q = [E]^{p+q} = [E_{p+q}] \end{array} \right\} \ldots . (5). $$

that is to say, making the suffixes of $[E]$ follow the index law, Equation (4) takes the following remarkably simple symbolic form :—

$$ E_n = ([E] + 1)^n \ldots \ldots (6). $$

Further, this Equation being *general* for all (positive integral) values of n—

$$\begin{aligned}E_p.E_q &= ([E]+1)^p.([E]+1)^q\\ &= ([E]+1)^{p+q}\\ &= E_{p+q} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).\end{aligned}$$

that is to say, the suffixes of E *also* follow the index law, so that the notation may be further modified with the interpretation—

$$(E)^p = E_p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

Hence, from Eq. (6) and (8)—

$$(E)^n = E_n = ([E]+1)^n \quad . \quad . \quad . \quad . \quad . \quad (9).$$

and this Equation being general for all (positive integral) values of n—

$$E = [E] + 1, \text{ and } [E] = E - 1 \quad . \quad . \quad . \quad . \quad (10).$$

These are symbolic relations between [E] and E of remarkable simplicity, and lead to an explicit formula for calculating $[E_n]$. For—

$$[E_n] = [E]^n = (E-1)^n.$$

$$= E_n - \frac{n}{1}.E_{n-1} + \frac{n(n-1)}{1.2}.E_{n-2} - \,.\,.\, + (-1)^r.\frac{\lfloor n}{\lfloor r.\lfloor n-r}.E_{n-r} +$$

$$.\,.\, + (-1)^{n-2}.\frac{n(n-1)}{1.2}.E_2 + (-1)^{n-1}.\frac{n}{1}.E_1 + (-1)^n \;.\;.\; (11).$$

IV. *Number of Elements in a Determinant out of whose n Leading Constituents only m are Finite.*

Generalise the notation of Problem III. as follows:—

Let $[\Delta]$, $[\Delta']$, $[\Delta'']$, . . . $[\Delta^m]$ represent the values of Δ (an ordinary determinant with finite constituents), when out of its *leading* constituents (of type a_{pp}) there are respectively none, one, two, &c., ... *m finite*, and the remainder zero.

Let $[E_n]$, $[E'_n]$, $[E''_n]$, . . . $[E^m_n]$ represent the corresponding values of E_n: by this notation $[E^n_n] = E_n$.

Then, by the same reasoning as in Problem III., and noting (in addition) that all terms involving the continued product of *more* than m leading constituents vanish necessarily in the case of the determinant $[\Delta^m]$, it follows that—

$$[\Delta^m] = [\Delta] + \Sigma\left\{a_{pp}.\left[\frac{d\Delta}{da_{pp}}\right]\right\} + \Sigma\left\{a_{pp}\,a_{qq}.\left[\frac{d^2\Delta}{da_{pp}.da_{qq}}\right]\right\} + \,.$$

$$+ \Sigma\left\{(a_{11}a_{22}a_{33}\;.\;.\;a_{mm}).\left[\frac{d^m\Delta}{da_{11}.da_{22}.da_{33}\;.\;.\;.\;da_{mm}}\right]\right\} \;.\; (12).$$

$$\therefore [E^m_n] = [E_n] + {}^mC_1 . [E_{n-1}] + {}^mC_2 . [E_{n-2}] + . + {}^mC_r . [E_{n-r}] + \ldots + {}^mC_{m-1} . [E_{n-m+1}] + [E_{n-m}] .$$

$$= [E_n] + \frac{m}{1} . [E_{n-1}] + \frac{m(m-1)}{1 . 2} . [E_{n-2}] + \ldots + \frac{\lfloor m}{\lfloor r . \lfloor n-r} . [E_{n-r}] + \ldots + \frac{m}{1} . [E_{n-m+1}] + [E_{n-m}] . \quad . \quad . \quad (13).$$

Next, changing every term $[E_p]$ into its symbolic equivalent $[E]^p$, see Eq. (5), $[E]^{n-m}$ is seen to be a factor in every term of series (13), which may, therefore, be symbolically expressed—

$$\left. \begin{aligned} [E^m_n] &= [E_{n-m}] . \{ [E] + 1 \}^m \\ &= [E_{n-m}] . E_m , \text{ (See Eq. 6).} \end{aligned} \right\} . \quad . \quad . (14).$$

Hence also, by changing m into $(n-m)$—

$$\left. \begin{aligned} [E^{n-m}_n] &= [E_m] . \{ [E] + 1 \}^{n-m} \\ &= [E_m] . E_{n-m} , \text{ (See Eq. 6).} \end{aligned} \right\} . \quad . \quad . (15).$$

Again changing $[E]$ into its equivalent $(E-1)$, by Eq. (10), and interpreting the result by Eq. (8).

$$[E^{n-m}_n] = (E-1)^m . E_{n-m}$$

$$= E_n - \frac{m}{1} . E_{n-1} + \frac{m(m-1)}{1 . 2} . E_{n-2} - \ldots + (-1)^r . \frac{\lfloor m}{\lfloor r . \lfloor m-r} . E_{n-r} + \ldots + (-1)^{m-1} . \frac{m}{1} . E_{n-m+1} + (-1)^m . E_{n-m} . \quad . \quad (16).$$

Formulæ (13) and (16) furnish the means of calculating $[E^m_n]$ or $[E^{n-m}_n]$ in terms of $[E]$ or E respectively. The former is preferable if $m < n \div 2$, and the latter if $m > n \div 2$.

The following particular instances of these expansions may be recorded for reference :—

$$\left. \begin{aligned} [E'_n] &= [E_n] + [E_{n-1}] ; \\ [E^{n-1}_n] &= E_n - E_{n-1} \\ [E''_n] &= [E_n] + 2 [E_{n-1}] + [E_{n-2}] ; \\ [E^{n-2}_n] &= E_n - 2E_{n-1} + E_{n-2} \\ [E'''_n] &= [E_n] + 3 [E_{n-1}] + 3 [E_{n-2}] + [E_{n-3}] ; \\ [E^{n-3}_n] &= E_n - 3E_{n-1} + 3E_{n-2} - E_{n-3} \end{aligned} \right\} (17).$$

The following relations between successive values of $[E^m_n]$ and also $[E^{m-n}_n]$ might be obtained by induction from the above equations, viz., that—

$$\left. \begin{aligned} [E^m_n] &= [E^{m-1}_n] + [E^{m-1}_{n-1}] \\ [E^{n-m}_n] &= [E^{n-m+1}_n] - [E^{n-m}_{n-1}] \end{aligned} \right\} (18).$$

but are more readily obtained in a *general* manner by symbolic work, thus—

$$[E_n^{m-1}] + [E_{n-1}^{m-1}] = [E_{n-m+1}].E_{m-1} + [E_{n-m}].E_{m-1} \text{ by Eq. (14).}$$
$$= [E_{n-m}].E_{m-1}.\{[E]+1\} \text{ by Eq. (5).}$$
$$= [E_{n-m}].E_m = [E_n^m] \text{ by Eq. (10, 7, and 14).}$$
$$[E_n^{n-m+1}] - [E_{n-1}^{n-m}] = [E_{m-1}].E_{n-m+1} - [E_{m-1}].E_{n-m} \text{ by (15).}$$
$$= [E_{m-1}].E_{n-m}.(E-1) \text{ by Eq. (7).}$$
$$= [E_m].E_{n-m} = [E_n^{n-m}] \text{ by Eq. (10, 5, \& 15).}$$

V. *Addendum to Problems* (III.) *and* (IV.)

The expressions (11) and (16) for $[E_n]$ and $[E_n^{n-m}]$ in terms of E obtained by a *symbolic* inversion of the formulæ (4) and (13) previously obtained for E_n and $[E_n^m]$ in terms of $[E_n]$ may also be obtained by *algebraic* inversion of the same formulæ, but the process is very tedious. They may also be obtained *directly* from the properties of determinants by establishing expressions for $[\Delta]$ and $[\Delta^{n-m}]$ in terms of Δ inverse to the expressions (3) and (12) used in the text.

The required expressions are easily seen to be—

$$[\Delta] = \Delta - \Sigma\left\{a_{pp}.\frac{d\Delta}{da_{pp}}\right\} + \Sigma\left\{a_{pp}a_{qq}.\frac{d^2\Delta}{da_{pp}.da_{qq}}\right\} - \Sigma\left\{a_{pp}a_{qq}a_{rr}.\frac{d^3\Delta}{da_{pp}.da_{qq}.da_{rr}}\right\} + + \ldots + \ldots + (-1)^n.(a_{11}a_{22}a_{33}\ldots a_{nn}) \ldots (19).$$

$$[\Delta^{n-m}] = \Delta - \Sigma\left\{a_{pp}.\frac{d\Delta}{da_{pp}}\right\} + \Sigma\left\{a_{pp}a_{qq}.\frac{d^2\Delta}{da_{pp}.da_{qq}}\right\} \ldots + (-1)^m.\Sigma\left\{a_{11}a_{22}a_{33}\ldots a_{mm}.\frac{d^m\Delta}{da_{11}.da_{22}.da_{33}\ldots da_{mm}}\right\} (20).$$

Equations (11) and (16) may be derived from Eq. (19) and (20) respectively by considerations precisely similar to those used in obtaining Eq. (4) and (13) from Eq. (3) and (12) respectively in the text.

The relations established in Problems (III.) and (IV.) between E_n, $[E_n]$, and $[E_n^m]$ are evidently derived in a manner which shows them to be *generally* true of all determinants whatever which are related to one another *similarly* to those styled Δ, $[\Delta]$, $[\Delta^m]$, *i.e.*, *differing only in their leading constituents* (those of Δ being all finite, those of $[\Delta]$ all zero, those of $[\Delta^m]$ being m finite, and the rest zero).

VI. *Application to Ordinary Determinants.*

Substituting $E_p = \lfloor p$, (Eq. 2) into Eq. (11), there results—

$$\left.\begin{aligned}[E_n] &= \lfloor n.\left\{1 - \frac{1}{\lfloor 1} + \frac{1}{\lfloor 2} - \frac{1}{\lfloor 3} + \ldots + \frac{(-1)^r}{\lfloor r} + \right. \\ &\qquad \left. \ldots\ldots + \frac{(-1)^{n-1}}{\lfloor n-1} + \frac{(-1)^n}{\lfloor n}\right\} \ldots \\ &= \lfloor n.\Sigma_0^n \frac{(-1)^x}{\lfloor x} \ldots\ldots\ldots\ldots \end{aligned}\right\} (21).$$

A relation between three successive values of $[E_n]$ may be thus found by Eq. (21)—

$$\begin{aligned}[E_{n-1}] + [E_{n-2}] &= \lfloor n-1.\Sigma_0^{n-1}\frac{(-1)^x}{\lfloor x} + \lfloor n-2.\Sigma_0^{n-2}\frac{(-1)^x}{\lfloor x} \\ &= \lfloor n-1.\frac{(-1)^{n-1}}{\lfloor n-1} + \left\{(n-1)+1\right\}.\lfloor n-2.\Sigma_0^{n-2}\frac{(-)1^x}{\lfloor x}\end{aligned}$$

$$\begin{aligned}\therefore (n-1).\{[E_{n-1}] + [E_{n-2}]\} &= (-1)^{n-1}.(n-1) + \lfloor n.\Sigma_0^{n-2}\frac{(-1)^x}{\lfloor x} \\ &= \lfloor n.\left\{\frac{(-1)^n}{\lfloor n} + \frac{(-1)^{n-1}}{\lfloor n-1} + \Sigma_0^{n-2}\frac{(-1)^x}{\lfloor x}\right\} \\ &= \lfloor n.\Sigma_0^n\frac{(-1)^x}{\lfloor x} \\ &= [E_n], \ldots \text{by Eq. (20)} \ldots (22).\end{aligned}$$

This relation (22) between three successive values of $[E_n]$ may also be thus obtained *directly* :—

Since the leading constituents of $[\Delta]$ are all *zero* (by definition)—

$$\therefore [\Delta] = \frac{1}{n}.\Sigma\left\{a_{yz}.\frac{d[\Delta]}{da_{yz}}\right\} \ldots\ldots\ldots (23).$$

in which equation y, z take all positive integral values (except equal values) from 1 to n. Now $\frac{d[\Delta]}{da_{yz}}$ is easily seen to be a determinant of $(n-1)$ rows containing $(n-2)$ of the leading (evanescent) constituents of the original determinant $[\Delta]$, and no *two* of these in the same row or column. These $(n-2)$ constituents may by change of order of rows or columns of the determinant $\frac{d[\Delta]}{da_{yz}}$ be all brought into its leading diagonal without altering its *numerical* value (the only alteration being of sign). It follows that the number of elements in $\frac{d[\Delta]}{da_{yz}}$ is the same as in a determinant of $(n-1)$ rows with only *one* finite leading constituent, which number (by notation of Problem IV.) is $[E'_{n-1}]$.

Further, the number of terms in the sum in Result (23) is clearly the same as the number of constituents of type a_{yz} in $[\Delta]$, *i.e.* (n^2-n), see par. 4, Problem I.

Hence, since $[E_n]$ is (by the notation) the number of elements in $[\Delta]$, it follows from (23) that—

$$[E_n]=\frac{1}{n}.(n^2-n).[E'_{n-1}]$$
$$=(n-1).\{[E_{n-1}]+[E_{n-2}]\}, \text{ by Eq. (17)} \quad . \; . \; (22).$$

Thus, the value of $[E_n]$ may be calculated for *successive* values of n by formula (22) from the known values of $[E_1]$, $[E_2]$, &c., or may be directly calculated from the series (21).

It will be useful to record a few values of $[E_n]$ for reference, thus—

$$\left.\begin{array}{l}[E_1]=0,\ [E_2]=1,\ [E_3]=2,\ [E_4]=9,\ [E_5]=44, \\ \qquad [E_6]=265,\ [E_7]=1854,\ [E_8]=14{,}833, \\ \qquad [E_9]=133{,}496,\ [E_{10}]=1{,}334{,}961.\end{array}\right\} \; . \; . \; (24).$$

Corollary. Substituting for $[E_n]$ from Eq. (21) into Eq. (4) and (2), and separating the symbols of operation and quantity—

$$\underline{|n}=E_n=\underline{|n}.\left\{\begin{array}{l}\Sigma_o^n+\frac{1}{\underline{|1}}.\Sigma_o^{n-1}+\frac{1}{\underline{|2}}\Sigma_o^{n-2}+\ldots \\ +\frac{1}{\underline{|r}}.\Sigma_o^{n-r}+\ldots.\frac{1}{\underline{|n-1}}.\Sigma_o^1+\frac{1}{\underline{|n}}.\Sigma_o^0\end{array}\right\}.\frac{(-1)^x}{\underline{|x}},$$

from which may be deducted the two sums—

$$\Sigma_o^n\frac{1}{\underline{|x}}.\Sigma_o^{n-x}\frac{(-1)^x}{\underline{|x}}=1=\Sigma_o^n\frac{1}{\underline{|n-x}}\Sigma_o^x\frac{(-1)^x}{\underline{|x}} \quad . \; . \; . \; . \; (25).$$

It is easy to see that all the terms of the series (21) for $[E_n]$ are even integers except the two last, which are $(-1)^{n-1}(n-1)$, so that $[E_n]$ will be an integer, and odd or even according as n is even or odd.

VII. *Number of Elements which are Products of $n\div 2$ Pairs of Conjugate Constituents in a Determinant whose Leading Constituents Vanish.*

This problem is a preliminary to the problem of finding the number of elements in a symmetric determinant.

By "conjugate constituents" are meant pairs of type a_{yz}, a_{zy}.

The type of element in question is—

$$\{(a_{pq}.a_{qp}).(a_{rs}.a_{sr}) \; . \; . \; . \; . \; . \; (a_{yz}.a_{zy})\}$$

containing $n\div 2$ products of pairs of conjugate constituents, such as $(a_{yz}.a_{zy})$. This element may be separated into two conjugate factors, viz. $(a_{pq}.a_{rs}. \; . \; . \; . \; a_{yz}).(a_{qp}.a_{sr}. \; . \; . \; . \; a_{zy})$

either of which involves the other, and *each* of which is the product of $n \div 2$ constituents involving *all* the suffixes *without repetition.* Hence the number of such elements is the same as the number of ways in which a product of $n \div 2$ constituents can be formed involving *all* the n suffixes *without repetition.*

Let S_n be the number of products containing n suffixes *without repetition.*

Now the number of constituents containing any particular suffix p is clearly $(n-1)$, for these constituents are of type A_{py}, where y has every value from 1 to n, *excepting* p. Also for *every* such constituent as a_{py}, there are $(n-2)$ suffixes remaining to form the remaining $\left(\frac{n}{2}-1\right)$ constituents required to form the complete product of $n \div 2$ constituents. Further, these $(n-2)$ suffixes can be arranged into the required product of $\left(\frac{n}{2}-1\right)$ constituents of the requisite type in S_{n-2} ways (by preceding notation).

Hence $S_n = (n-1).\ S_{n-2}$.

Similarly $S_{n-2} = (n-3).S_{n-4}$, and so on.

$$\left.\begin{aligned} \text{Hence } S_n &= (n-1).S_{n-2} = (n-1).(n-3)S_{n-4} = \ \ldots \\ &= (n-1).(n-3).(n-5) \ \ldots\ 7.5.3.S_2 \ \ldots \\ &= (n-1).(n-3).(n-5) \ \ldots\ 7.5.3.1 \ \ldots \end{aligned}\right\} (26).$$

since obviously when $n=2$, there is only one pair of conjugate constituents (viz., a_{12}, a_{21}), so that $S_2 = 1$

$$\left.\begin{aligned} \therefore S_n &= \frac{n(n-1)(n-2)(n-3)(n-4)\ \ldots\ 7.6.5.4.3.2.1}{n.\quad (n-2)\quad (n-4).\ \ldots\ 6.4.2} \\ &= \frac{\lfloor n}{2^{\frac{n}{2}}.\lfloor \frac{n}{2}}, \text{ where } n \text{ is an } \textit{even} \text{ integer.} \end{aligned}\right\} \ldots (27).$$

It is obvious that, if n be an *odd* number, the proposed product of $n \div 2$ pairs of conjugate constituents could not be formed, *i.e.* that *no* elements exist of type proposed.

$\therefore S_n = o$, when n is an *odd* integer . . . . (28).

A few values of S_n may be recorded for reference, and note that S_n is always an odd integer (when n is even), being itself the product of odd integers, Eq. (26).

$S_0 = 1,\ S_2 = 1,\ S_4 = 3,\ S_6 = 15,\ S_8 = 105,\ S_{10} = 945 \ \ldots$ (29).

VIII. *Number of Elements in a Symmetric Determinant.*

Modify the preceding notation in capital letters for ordinary determinants by using the corresponding small letters with like meanings for symmetric determinants.

Thus $\Delta, [\Delta], [\Delta^m], E_n, [E_n], [E^m_n]$ become—

$$\delta, [\delta], [\delta^m], e_n, [e_n], [e^m_n].$$

All the relations between $E_n, [E_n], [E^m_n]$ established in Problems (III.) and (IV.) obtain also between e_n, $[e_n]$, $[e^m_n]$, having been established in a general manner, as properties common to all determinants.

The number of elements $[e_n]$ in the symmetric determinant $[\delta]$, whose leading constituents vanish, will first be investigated.

Number of Elements in a Symmetric Determinant whose Leading Constituents are Zero.

In the *ordinary* determinant $[\Delta]$ whose leading constituents vanish, the elements may be divided into two classes:—

(1). Of type $\{(a_{pq}.a_{\ p}).(a_{rs}a_{sr.})\ .\ .\ .\ (a_{yz}.a_{zy})\}$ consisting of the product of $n\div 2$ pairs of *conjugate* constituents (such as $(a_{yz}.a_{zy})$ only. This number has been investigated in Problem (VII.), and denoted by S_n.

(2). Of type—

$\{(a_{pq}.a_{qp}).(a_{rs}a_{sr})\ .\ .\ .\ (a_{yz}.a_{zy})\} \times \{a_{fg}.a_{gh}.a_{hi.}\ .\ .\ .\ .a_{mf.}\}$,

consisting of the product of r pairs of *conjugate* constituents (such as $a_{yz}.a_{zy}$), and $(n-2r)$ other constituents (containing *no* conjugate pair; that is to say, consisting in part at least of the product of constituents containing *no* conjugate pair. The number of this type is clearly $\{[E_n]—S_n\}$, $[E_n]$ being (by the notation) the whole number of elements in the determinant $[\Delta]$.

In the corresponding *symmetric* determinant $[\delta]$, in which (by definition) $a_{yz}=a_{zy}$, these classes become—

(1). Of type $\{(a_{pq}.a_{rs}.\ .\ .\ .\ .\ a_{yz}\}^2$, each element being the square of the product of $n\div 2$ constituents, *without repetition of any suffix.* Also the number of these is clearly the same, viz., S_n as in the corresponding ordinary determinant $[\Delta]$.

(2). Of type $\{a_{pz}.a_{rs}.\ .\ .\ .\ a_{yz}\}^2 \times \{a_{fg}.a_{gh}.a_{hi.}\ .\ .\ .\ a_{mf}\}$ consisting of the product of the square of the product of r constituents, *without repetition of any suffix*, and the product of $(n—2r)$, other constituents containing no conjugate pair, which therefore involves the remaining $(n—2r)$ suffixes in a cyclic change, *i.e.*, *each occurring twice.*

Now in the ordinary determinant $[\Delta]$, elements of this type (2) occur in pairs, *i.e.*, for *every* product of a particular set of conjugate constituents, as $\{(a_{pq}.a_{qp}).(a_{rs}.a_{sr.})\ .\ (a_{yz}.a_{zy})\}$,

there is a *pair of conjugate products* of $(n-2r)$ other constituents, viz. $\{a_{fg}.a_{gh}\,a_{hi}.\ldots.a_{mf}\}$ and $\{a_{fm}.\ldots a_{ih}.a_{hg}.a_{gf}.\}$, so that the type of *the sum of a pair* of such conjugate elements is $\{(a_{pq}.a_{qp}).(a_{rs}\,a_{sr})\ldots(a_{yz}a_{zy}.)\}\times[\{a_{fg}.a_{gh}.a_{hi}\ldots a_{mf}\}+\{a_{fm}.\ldots a_{ih}.a_{hg}.a_{gf}\}]$, which pair reduces in the symmetric determinant $[\delta]$ to a single element of type—

$$2\{a_{pq}.a_{rs}\ldots\ldots a_{yz}\}^2.[a_{fg}.a_{gh}.a_{hi}.\ldots\ldots a_{mf}.]$$

since by definition $a_{yz}=a_{zy}$. The number of elements of this type in the symmetric determinant $[\delta]$ is therefore one-half that in the ordinary determinant $[\Delta]$, *i.e.* is $\frac{1}{2}\{[E_n]\{-S_n\}$,

Adding the number of both classes together, there results—

$$[e_n]=S_n+\tfrac{1}{2}\{[E_n]-S_n\}=\tfrac{1}{2}\{[E_n]+S_n\}\ \ldots\ldots\ldots\ (30).$$

substituting for $[E_n]$ and S_n from formulæ (21), (27), (28),

$$\left.\begin{aligned}[e_n]&=\tfrac{1}{2}\ [E_n] &&=\frac{\lfloor n}{2}.\Sigma_0^n\frac{(-1)^x}{\lfloor x} &&\text{when } n \text{ is odd}\\ [e_n]&=\tfrac{1}{2}\{[E_n]+S_n\} &&=\frac{\lfloor n}{2}.\left\{\Sigma_0^n\frac{(-1)^x}{\lfloor x}+\frac{1}{2^{\frac{n}{2}}.\lfloor\frac{n}{2}}\right\} &&\text{when } n \text{ is even}\end{aligned}\right\}(31).$$

Also, since $[E_n]$ is known to be an even integer when n is odd, and an odd integer when n is even (see Problem VI.), and since also S_n is an odd integer when n is even, it is easily seen from (31) that $[e_n]$ is always an integer (as of course it should be).

A relation between three successive values of $[e_n]$ may be deduced from that between three successive values of $[E_n]$, see Eq. (22)—

Thus $[E_n]=(n-1).\{[E_{n-1}]+[E_{n-2}]\}$

$$\therefore\ 2[e_n]-S_n^?=(n-1).\{2[e_{n-1}]+$$
$$+2[e_{n-2}]-S_{n-1}-S_{n-2}\},\ \text{by Eq. (30).}$$

And when n is odd—

$$S_n=0,\ S_{n-1}=\frac{\lfloor n-1}{2^{\frac{n-1}{2}}.\lfloor\frac{n-1}{2}},\ S_{n-2}=0,\ \text{by (27), (28).}$$

And when n is even $S_n=(n-1).S_{n-2}$, $S_{n-1}=0$, by (26), (28).

$$\left.\begin{aligned}\therefore\ [e_n]&=(n-1).\{[e_{n-1}]+[e_{n-2}]\}-\frac{n-1}{2}.S_{n-1}\\ &\qquad\text{when } n \text{ is odd}\\ [e_n]&=(n-1).\{[e_{n-1}]+[e_{n-2}]\}\\ &\qquad\text{when } n \text{ is even}\end{aligned}\right\}\ \ldots\ldots\ (32).$$

Thus the value of $[e_n]$ may be calculated for *successive*

values of n by formula (32), from the known values of $[e_1]$, $[e_2]$, &c., or may be directly calculated from formulæ (31).

It will be useful to record a few values of $[e_n]$ for reference—

$$\left.\begin{array}{r}[e_0]=1,[e_1]=0\,[e_2]=1,[e_3]=1,[e_4]=6,[e_5]=22,\\ [e_6]=140,[e_7]=927,[e_8]=7469,[e_9]=66{,}748,\\ [e_{10}]=667{,}953\end{array}\right\}\ldots\ (33).$$

IX. *Numbers of Elements in a Symmetric Determinant.*

This may easily be calculated from the known number $[e_n]$ of elements in the symmetric determinant $[\delta]$, whose leading constituents are zero, by help of the relation between e and $[e]$ (see Eq. 6).

Thus $e_n=(1+[e])^n$

$$=1+\frac{n}{1}[e_1]+\frac{n(n-1)}{1.2}[e_2]+\ldots+\frac{\lfloor n}{\lfloor r.\lfloor n-r}[e_r]+\ldots$$
$$+\frac{n}{1}[e_{n-1}]+[e_n]\ \ldots\ (34).$$

It will be useful to record a few values of e_n for reference, thus—

$$\left.\begin{array}{l}e_0=1,\ e_1=1,\ e_2=2,\ e_3=5,\ e_4=17,\ e_5=73,\ e_6=398,\\ e_7=2636,\ e_8=20{,}542,\ e_9=182{,}750,\ e_{10}=1{,}819{,}148\end{array}\right\}\ldots(35).$$

X. *Number of Elements in a Symmetrical Determinant, out of whose Leading Constituents only m are Finite.*

This may be easily calculated, either directly from the relations (13) and (16), or for successive values of n from the relations (18) which have established for all determinants. Thus changing E to e—

$$[e^m_n]=[e_n]+\frac{m}{1}.[e_{n-1}]+\frac{m(m-1)}{1.2}.[e_{n-2}]+\ldots$$
$$+\frac{\lfloor m}{\lfloor r.\lfloor m-r}.[e_{n-r}]+\ldots+\frac{m}{1}.[e_{n-m+1}]+[e_{n-m}]\ \ldots(36).$$

$$[e^{n-m}_n]=e_n-\frac{m}{1}.e_{n-1}+\frac{m(m-1)}{1.2}.e_{n-2}+\ldots+\frac{(-1)^r\lfloor m}{\lfloor r.\lfloor m-r}.e_{n-r}+\ldots$$
$$+(-1)^{m-1}\frac{m}{1}.e_{n-m+1}+(-1)^n e_{n-m}\ \ldots(37).$$

$$[e^m_n]=[e^{m-1}_n]+[e^{m-1}_{n-1}],\text{ and }[e^{n-m}_n]=[e^{n-m+1}_n]-[e^{n-m}_{n-1}]\ \ldots(38).$$

On account of the great use of symmetric determinants in modern geometry, it will be useful to record the values of $[e^m_n]$ in a few cases. Thus, observing that $[e^0_n]=[e_n]$, and that $[e^n_n]=[e_n]$, (by notation)—

Table of Values of $[e_n^m]$ ...... (39).

n	m: o	i.	ii.	iii.	iv.	v.	vi.	vii.	viii.	ix.	x.
o	1										
1	o	1									
2	1	1	2								
3	1	2	3	5							
4	6	7	9	12	17						
5	22	28	35	44	56	73					
6	140	162	190	225	269	325	398				
7	927	1067	1229	1419	1644	1913	2238	2636			
8	7469	8396	9463	10692	12111	13755	15668	17906	20542		
9	66748	74217	82613	92076	102768	114879	128634	144302	162208	182750	
10	667953	734701	808918	891531	983607	1086375	1201254	1329888	1474190	1636398	1819184

XI. *Number of Elements in a Skew Symmetric Determinant.*

Denote the number of elements in a *skew symmetric* determinant of n rows by $[\varepsilon_n]$, the brackets being used to preserve the analogy with previous notation for determinants whose leading constituents are zero.

It is shown (Salmon, Art. 37) that every skew symmetric determinant of odd order vanishes; the number of its elements is therefore zero.

It is shown (Salmon, Art. 38, 39) that every skew symmetric determinant of even order is a perfect square, and may be expressed by $\{\Sigma(\pm a_{pq} \,.\, a_{rs} \ldots . \, a_{yz})\}^2$, *i.e.*, by the square of the sum of terms of type $(\pm a_{pq} \,.\, a_{rs} \ldots . a_{yz})$, each of which is the product of $n \div 2$ constituents, involving *all* the suffixes *without repetition*. It has been shown (Problem VII.), that the number (S_n) of such products in a determinant whose leading constituents vanish (as is the case in a skew symmetric determinant, see Salmon, Art. 37) is

$$S_n = \underline{|n} \div \left(2^{\frac{n}{2}} . \underline{\left|\tfrac{n}{2}\right.}\right).$$

Also, since the determinant is the square of S_n different terms, it follows that the number of its elements $[\varepsilon_n]$ is the same as the number of terms in the expanded square of the sum of S_n quantities.

$$\therefore [\varepsilon_n] = S_n + \left\{ \begin{array}{l} \text{(number of combinations of} \\ \quad S_n \text{ things two together).} \end{array} \right.$$

$$= S_n + \tfrac{1}{2} S_n \,.\, (S_n - 1)$$

$$\left.\begin{array}{l} = \tfrac{1}{2} S_n \,.\, (S_n + 1), \\ = \dfrac{\underline{|n} . (\underline{|n} + 2^{\frac{n}{2}} . \underline{\left|\tfrac{n}{2}\right.})}{2^{n+1} . (\underline{\left|\tfrac{n}{2}\right.})^2} \end{array}\right\} \text{when } n \text{ is } \textit{even} \ldots . (40).$$

And $[\varepsilon_n] = 0$, when n is *odd*, (v. supra) .... (41).

Eq. (40) shows that $[\varepsilon_n]$ is always an integer (as it should be). It will be useful to record a few values of $[\varepsilon_n]$ for reference. Thus—

$$\left.\begin{array}{r}[\varepsilon_0]=1,\ [\varepsilon_2]=1,\ [\varepsilon_4]=6,\ [\varepsilon_6]=120,\ [\varepsilon_8]=5565,\\ [\varepsilon_{10}]=446{,}985\end{array}\right\}\ldots\ldots(42).$$

XII. *Number of Elements in a Skew Determinant.*

Denote the number of elements in a skew determinant of n rows by ε_n. Then, since a skew symmetric determinant is a skew determinant whose leading constituents are zero, the relation between them is the same as between $[\Delta]$ and Δ, so that the relations between $[\varepsilon_n]$ and ε_n will be the same as those between $[E_n]$ and E_n demonstrated under Problems (III.) and (IV.) as common to all determinants. Thus, the values of ε_n may be calculated from Eq. (6), viz.:—

$$\begin{aligned}\varepsilon_n &= (1+[\varepsilon]^n)\\ &= 1+\frac{n}{1}\cdot[\varepsilon_1]+\frac{n(n-1)}{1.2}\cdot[\varepsilon_2]+\ldots+\frac{\lfloor n}{\lfloor r\cdot\lfloor n-r}\cdot[\varepsilon_r]+\ldots\\ &\qquad +\frac{n}{1}[\varepsilon_{n-1}]+[\varepsilon_n]\ldots\ldots(43).\end{aligned}$$

A few values of ε_n may be recorded for reference—

$$\left.\begin{array}{r}\varepsilon_0=1,\ \varepsilon_1=1,\ \varepsilon_2=2,\ \varepsilon_3=4,\ \varepsilon_4=13,\ \varepsilon_5=41,\ \varepsilon_6=226,\\ \varepsilon_7=1072,\ \varepsilon_8=9374,\ \varepsilon_9=60{,}968,\ \varepsilon_{10}=723{,}966\end{array}\right\}\ldots\ldots(44).$$

It is also seen that if $[\varepsilon_n^m]$ denote the number of elements in a skew determinant out of whose leading constituents only m are finite, then changing E in Problem (IV.) into ε, all the relations demonstrated under Problem (IV.) between E_n, $[E_n]$, and $[E_n^m]$ are true between ε_n, $[\varepsilon_n]$, and $[\varepsilon_n^m]$, so that $[\varepsilon_n^m]$ may be calculated from the known ε_n, $[\varepsilon_n]$ by the formulæ of Problem (IV.).

XIII. *Number of Minors in a Determinant.*

By definition, a pth minor is formed by erasing p rows and p columns from the original, so that evidently :—

$$\left.\begin{array}{l}\text{A } p\text{th minor is a determinant of } (n-p) \text{ rows and columns}\\ \text{A } (n-p)\text{th minor is a determinant of } p \text{ rows and columns}\end{array}\right\}$$

Let ${}^n M_p$ be the number of pth minors that can be formed from a determinant of n rows. Then ${}^n M_{n-p}$ is the number of $(n-p)$th minors.

It is obvious that, in this notation, the minor of zero order ($p=0$) being the original determinant itself, and the minors of nth order being zero—

$${}^n M_o = 1, \text{ and } {}^n M_n = 1$$

in *all* determinants (which are finite) . . . . (45).

Now ^nC_r being the number of combinations of n things taken r together (as in Problem III.), it is clear that ^nC_p is the number of ways in which p rows can be selected out of n rows, also that ^nC_p is the number of ways in which p columns can be selected out of n columns. But ^nM_p is evidently the number of ways in which *any* p rows and *any* p columns can be selected *simultaneously* (for erasion) from the n rows and n columns in the determinant.

$$\therefore\ ^nM_p = {^nC_p} \times {^nC_p} = ({^nC_p})^2 = \left(\frac{\underline{|n}}{\underline{|p}\cdot\underline{|n-p}}\right)^2 \ldots\ldots (46).$$

Eq. (46) shows that, as is also evident from the reasoning itself—

$$^nM_p = {^nM_{n-p}} \ldots\ldots\ldots\ldots\ldots (47).$$

From the preceding reasoning, it may also be inferred that result (47) is a property common to all determinants whose minors are *all* finite (except in certain cases when these numbers ^nM_p, $^nM_{n-p}$ are unequally reduced, in consequence of the constituents being so related as to produce equality among some of the minors).

As a particular case of Eq. (47), $^nM_1 = {^nM_{n-1}}$, *i.e.*, "The first minors are the same in number as the constituents (these being actually the $n-1$th minors)."

XIV. *Number of Minors in a Symmetric Determinant.*

In *symmetric* determinants it is clear that there is only *one* way in which a particular leading minor (which is itself also a symmetric determinant) can be selected, but that any other minor can always be selected in *two* ways, *e.g.*, the *same* (non-leading) pth minor may be formed either by omitting a certain set of p rows and p columns from the original determinant, or by omitting the conjugate set of p columns and p rows. This amounts to saying that the *non-leading* minors occur in pairs of equal magnitude.

Now the number of leading pth minors being those minors which contain $(n-p)$ leading constituents of the original determinants in their own leading diagonals, is clearly equal to the number of ways in which $(n-p)$ constituents can be selected from the whole n leading constituents, *i.e.*, is equal to $^nC_{n-p}$ or ^nC_p.

Also, the number of *non-leading* pth minors would in an *ordinary* determinant be (Problem XIII.) $\{({^nC_p})^2 - {^nC_p}\}$, which reduces in a *symmetric* determinant to $\frac{1}{2}\{({^nC_p})^2 - {^nC_p}\}$ for

the reasons above. Hence the whole number of pth minors is—

$$\left.\begin{aligned} {}^{n}\mathrm{M}_p &= {}^{n}\mathrm{C}_p + \tfrac{1}{2}\{({}^{n}\mathrm{C}_p)^2 - ({}^{n}\mathrm{C}_p)\} \\ &= \tfrac{1}{2}\,{}^{n}\mathrm{C}_p\,({}^{n}\mathrm{C}_p + 1), \text{ which is clearly an integer.} \\ &= \frac{\underline{|n}}{2}\cdot\frac{\underline{|n} + \underline{|p}\cdot\underline{|n-p}}{(\underline{|p}\cdot\underline{|n-p})^2} \end{aligned}\right\}\ldots (48).$$

Since ${}^{n}\mathrm{C}_p = {}^{n}\mathrm{C}_{n-p}$, therefore in symmetric determinants—

$${}^{n}\mathrm{M}_p = {}^{n}\mathrm{M}_{n-p} \ldots\ldots\ldots (49).$$

On account of the great use of symmetric determinants in modern geometry, it will be useful to record some values of ${}^{n}\mathrm{M}_p$ for reference.

N.B. In general
$$\left.\begin{aligned} {}^{n}\mathrm{M}_0 &= 1 = {}^{n}\mathrm{M}_n, \\ {}^{n}\mathrm{M}_1 &= \tfrac{1}{2}n(n+1) = {}^{n}\mathrm{M}_{n-1} \\ {}^{n}\mathrm{M}_2 &= \tfrac{1}{8}n.(n-1)(n^2-n+2) = {}^{n}\mathrm{M}_{n-2} \end{aligned}\right\}. (50).$$

Values of ${}^{n}\mathrm{M}_p$ .... (51).

n.	Value of p.										
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
1	1	1									
2	1	3	1								
3	1	6	6	1							
4	1	10	21	10	1						
5	1	15	55	55	15	1					
6	1	21	120	210	120	21	1				
7	1	28	231	630	630	231	28	1			
8	1	36	406	1596	2485	1596	406	36	1		
9	1	45	666	3570	8001	8001	3570	666	45	1	
10	1	55	1035	7260	22155	31878	22155	7260	1035	55	

XV. *Number of Minors in a Skew Symmetric Determinant.*

Note that all leading minors are themselves skew symmetric determinants, so that all leading minors containing an *odd* number of rows vanish (Salmon, Art. 38). Now a pth minor contains $(n-p)$ rows, and the number of *leading* pth minors in a symmetric determinant is in general ${}^{n}\mathrm{C}_p = \frac{\underline{|n}}{\underline{|p}\cdot\underline{|n-p}}$ (see Problem XIV.). Hence, in a skew symmetric determinant, the number of *leading* pth minors is zero, or ${}^{n}\mathrm{C}_p$ according as $(n-p)$ is odd or even. And the number of *non-leading* pth minors will be the same as in a symmetric determinant in general, viz., $\frac{1}{2}.\{({}^{n}\mathrm{C}_p)^2 - {}^{n}\mathrm{C}_p\}$. Hence, in a skew symmetric determinant—

$$\left.\begin{array}{l} {}^nM_p = \tfrac{1}{2}\,.\,{}^nC_p\,.\,({}^nC_p - 1) = \dfrac{\underline{|n}}{2}\,.\,\dfrac{\underline{|n} - \underline{|p}\,.\,\underline{|n-p}}{(\underline{|p}\,.\,\underline{|n-p})^2}, \\ \qquad\qquad\qquad\qquad\qquad \text{when } (n-p) \text{ is odd} \\ {}^nM_p = \tfrac{1}{2}\,.\,{}^nC_p\,.\,({}^nC_p + 1) = \dfrac{\underline{|n}}{2}\,.\,\dfrac{\underline{|n} + \underline{|p}\,.\,\underline{|n-p}}{(\underline{|p}\,.\,\underline{|n-p})^2}, \\ \qquad\qquad\qquad\qquad\qquad \text{when } (n-p) \text{ is even} \end{array}\right\}\,.\ (52).$$

These quantities are evidently both integers (as they should be). Eq. (47) is true of this class of determinants only when p and $(n-p)$ are both odd or both even, which cannot occur when n is odd, but always happens when n is even. Thus—

$${}^nM_p = {}^nM_{n-p}, \text{ when } n \text{ is even} \ldots\ldots (53).$$

The following relation obtains between two successive values of ${}^nM_1$, the *higher* value of n being an *even* number; so that $(n—p) = (n—1)$, an odd number, and $(\overline{n—1}—p) = (n—2)$, an even number.

$$\therefore\ {}^nM_1 = \tfrac{1}{2}\,n\,.\,(n—1) = \tfrac{1}{2}(n—1)\,.\,(\overline{n—1} + n) = {}^{n-1}M_1, \text{ by (52)},$$
$$n \text{ being even} \ldots . (54).$$

Eq. (52) shows that, *in general*, Eq. (45) is true of this class of determinants only when finite, *i.e.*, only when of even order, thus—

$$\left.\begin{array}{l} {}^nM_o = 0 = {}^nM_n, \text{ when } n \text{ is odd.} \\ {}^nM_o = 1 = {}^nM_n, \text{ when } n \text{ is even.} \end{array}\right\} \ldots\ldots\ldots\ (55).$$

It will be useful to record a few values of ${}^nM_p$ for this class of determinant. Thus—

Values of ${}^nM_p$ . . . . (56).

n.	Value of p.										
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
1	0	0									
2	1	1	1								
3	0	6	3	0							
4	1	6	21	6	1						
5	0	15	45	55	10	0					
6	1	15	120	190	120	15	1				
7	0	28	210	630	595	231	21	0			
8	1	28	406	1540	2485	1540	406	28	1		
9	0	45	630	3570	7875	8001	3486	666	36	0	
10	1	45	1035	7140	22155	31626	22155	7140	1035	45	1

NOTICES OF BOOKS.

Darwinism and Design; or Creation by Evolution. By George St. Clair, F.G.S., M.A.I.A., &c. London: Hodder and Stoughton. 1873.

We have here yet another attempt at a reconciliation of theology with science; but it is one which differs in many respects from all that have gone before it. The doctrine of evolution, as explained by Spencer and Darwin, is accepted in its entirety, and no objection is made to the most extreme consequences which those authors deduce from it; but it is argued with much force and ingenuity that design, and the constant action of a Supreme Ruler, is not thereby rendered inconceivable or unnecessary. A condensed but exceedingly accurate and well-written account of the most recent views of evolution is given in the first part of the volume; and this is a great merit, seeing how incapable most theological writers are of avoiding either positive misrepresentation or a partial and one-sided statement of the teachings of evolutionists. The theological treatment of the question is, however, somewhat peculiar and heterodox, and we fear will not meet with a favourable acceptance from the religious world; and this may render Mr. St. Clair's book less generally useful in reconciling the modern Christian mind to the teachings of Darwin than it might otherwise have been. A short account of the author's mode of treating the subject, and of the peculiarities above referred to, may not be uninteresting.

Throughout the book we meet with expressions and arguments which show us that the Deity or Supreme Ruler spoken of by the author is not the being to whom those terms are applied either in philosophy or religion. It is not the "Absolute" of the philosophers; it is not the "Almighty" and "Omnipresent" deity of the Christian; but it is a being subject, like ourselves, to the laws of matter and motion,—having to recognise the "nature of things," but having infinite knowledge which enables him to make use of the universe and its "necessary laws" so as to work out his own purposes. This view, which appears to us an impossible or at least an imperfect one, seems to have been adopted owing to the supposed "inconceivability" of the creation of matter out of nothing—an inconceivability which vanishes to any one who can thoroughly grasp the conception of matter as being essentially a complex set of forces and nothing else. To hold that these forces are eternal and self-existent, and that they produce by their varied interactions all the forms of dead matter, while an omniscient mind—equally eternal and self-existent—finds itself face to face with this matter and these forces which it can neither destroy nor originate, but only guide, is surely to

multiply difficulties. That the "forces" which we know as "matter" are in some way dependent on the supreme mind appears to us the only alternative to pure materialism; and this view renders it perfectly conceivable that "matter" may "be made out of nothing," or may be again resolved into nothing by the withdrawal of the mental action which is the sole cause of its existence. But if we conceive the material universe to be thus the product of the supreme mind, we may equally believe that there is a mental or spiritual universe, of which we ourselves form a part, and that the former is the means by which the latter is developed with the greatest capacity for happiness and eternal progress. Now Mr. St. Clair's deity is just such a being as we might suppose to be the highest in our material universe, and who might be charged with utilising to the utmost the powers of that universe in developing mind. He would "not violate natural law, but work by means of it," and his work would be liable to those "incidental results" often temporarily painful, injurious, or useless, which are such a stumbling-block to the usual ideas of divine government. The slow process of development—first of systems, then of worlds, then of matter into complex forms and qualities, and lastly, of organisation and life—has probably its own high uses, of which we can form no adequate conception. It may help the development of higher intelligences than ourselves; it may be the only mode by which multiplied forms of those higher intelligences can be produced. At all events, it is the system of nature; and it is hardly likely that any other possible system would be more intelligible to beings like ourselves—produced by it and still forming part of it.

Having thus indicated how we think Mr. St. Clair's theory may be made more consistent and more comprehensive, we proceed to give a few examples of his style of illustration and mode of reasoning. The argument against design from the existence of rudimentary organs or traces of structures that were useful only in ancestral forms, is answered by supposing a town, in which one of the main water-pipes goes half a mile into the country, and then bends back again with various windings for no useful purpose. "We ask where is the wisdom of carrying the water through this mile of pipe when it might go by the short cut? Why waste the tubing, and waste the time, and do what has to be undone immediately, in sending the stream to a point from which there is no course but to return? On the supposition that the town was originally built as it now stands, every street and square having the position they now have, and not a house more or less—our objection is valid. But if we learn that the diverging bend of pipe follows the route of streets which formerly existed, and that although the shorter cut would now seem better, yet it would cost more to take up the old pipes from the long route and lay down pipes on the short route than could possibly be gained by the process, we see the wisdom of leaving the arrangement

as it is; and we read in the existence of the bend of pipe a page of the past history of the town."

To the objection as to the existence of carnivorous animals in all ages of the earth's history, it is well replied that evolution shows struggle and death to be absolutely necessary to advancement and to render possible the eventual birth and perfecting of man. It is also very forcibly argued that there are many direct marks of beneficence in nature. So far as we know, there was no absolute need for life to appear at all on the earth; or, when it appeared, for it ever to have advanced beyond the lower forms; or for the distinction of male and female ever to have arisen; or for the eye to have the capacity of distinguishing sensations of colour as distinct from light and shade; or for the ear to be attuned so admirably to vibrations of the atmosphere as to render music possible; or for the taste to be capable of delighting in such an endless variety of savours. When we look at the whole range of past and present life upon the globe, what an infinite amount of pure enjoyment has been derived from every one of these faculties and powers, so that the existence of pain, which is the necessary correlative of many of them, counts for nothing in the balance. Even that endless variety, which seems a first principle of the material universe, so absolutely universal is it, adds in an incalculable degree to our enjoyment. It alone enables us to appreciate beauty, and it is almost certain that we should receive no pleasure from any of our senses if there were not an ever-varying series of objects and properties to excite them to various degrees and kinds of action. The conception that these almost infinite possibilities of enjoyment have come into existence as a necessary result of certain self-existent laws of self-existent matter, and have therefore not been in any way foreseen or designed by any intelligence, is one which seems too improbable to be permanently held by any thinker who will carefully examine the evidence from this point of view.

The last chapter—on the Moral Aspects of Evolution—is very well written, and deserves careful consideration. We have only space to notice what is termed the origin of moral species. The world has ever persecuted its reformers and put its prophets to death. The best, the wisest, and the most unselfish men have often left no posterity to inherit their good qualities. How, then, has the world advanced morally and intellectually? Mr. St. Clair imputes it to the generative action of mind upon mind, a more powerful agent in spreading truth and goodness than hereditary transmission. Each great mind acts upon all those which are somewhat lower than itself, and tends to raise them to its own level. "The man in whom the higher truth or higher virtue is first found may be said to constitute a new moral 'species' or 'variety.' The men who are nearest to him in the points in which he is distinguished are the species from which he probably has sprung, and being nearest to him would require

least alteration in themselves to make them quite like him. The influence fitted to produce this alteration is the presentation of his peculiarities before them in the example of his life or sufferings, in his verbal teaching or his written works. These, therefore, are the 'conditions of the environment' which induce 'variation' in a number of individuals, and convert them into the new species; it is not that offspring are generated in the parental likeness, but one species is evolved from another. Thus we have the wonderful fact that a new moral species can create the conditions which will cause others to vary into its likeness—the highest moral life agrees with the lowest physical life in possessing a protoplasmic power of multiplying itself indefinitely by contact. Not only has Natural Selection transferred its action to the mind, but the environing conditions which occasion the mental variations before they are selected have also to a large extent become mental." But this does not, as Mr. St. Clair remarks, explain how the variations arise which give purer conceptions and higher impulses to some men than to all the rest of mankind; neither does it explain why these dangerous gifts, often bringing persecution and death to their possessors, should have such a marvellous power of spreading, and prove so fascinating to many, to whom they will in all probability bring no better fate. He concludes that there is no other explanation but that truth and goodness have an immutable beauty proper to themselves, attractive to minds and consciences capable of perceiving the true relations of things; and that this can only be looked upon as due to the great Fount of all things.

We can cordially recommend this book to all who take an interest in the wider bearings of the doctrine of evolution. The writer is thoroughly imbued with the spirit of his subject, and even the experienced student will find much that is suggestive in the way in which the facts of well-known writers are presented and discussed. Some of the greater philosophical difficulties are, it is true, avoided rather than overcome; but we nevertheless feel that the book is well calculated to diminish anti-Darwinian prejudice, and to help forward the reconciliation of science with religion.

The Conservation of Energy. Being an Elementary Treatise on Energy and its Laws. By Balfour Stewart, M.A., F.R.S., Professor of Natural Philosophy at the Owen's College, Manchester. London: Henry S. King and Co. 1874. Crown, 8vo. 180 pp.

The Doctrine of the Conservation of Energy, which was indicated in Mr. Justice Grove's work on the "Correlation of the Physical Forces" some thirty years ago, has been considerably developed since the exact determination of the relationship which

exists between heat and mechanical work. Professor Stewart has written an admirable treatise on Heat, and has elsewhere discussed the Conservation of Energy with consummate ability. We are not disappointed by this more comprehensive treatment of the subject. He tells us succinctly in the preface his mode of discussing the subject. He divides our knowledge of the universe into two branches: the one knowledge of it as a vast physical machine composed of atoms swimming in the luminiferous ether; the other, the laws which regulate the working of this machine; in other words, the laws of energy.

In the first chapter, energy is defined as "the power of overcoming obstacles, or of doing work," as instanced by a rifle bullet in motion. The work is to be measured by some unit, preferably the kilogramme for weight, and the metre for height; and by multiplying a weight raised by the vertical height through which it is raised, we get the work done in kilogrammetres. Next we have various examples of the change of energy of position into energy of motion, and finally into heat. The usual and satisfactory examples of the head of water, the bent cross-bow, and the wound up watch are adduced, and the advantages of energy of position are exemplified by happily comparing a water-mill and a windmill with a rich and poor man. In the one case we may turn on the water whenever it is most convenient for us, but in the other we must wait until the wind happens to blow. The former has all the independence of a rich man; the latter all the obsequiousness of a poor one. If we pursue the analogy a step further, we shall see that the great capitalist, or the man who has acquired a lofty position, is respected because he has the disposal of a great quantity of energy; and whether he be a nobleman or a sovereign, or a general in command, he is powerful only from having something which enables him to make use of the services of others. When the man of wealth pays a labouring man to work for him, he is in truth converting so much of his energy of position into actual energy, just as a miller lets out a portion of his head of water in order to do some work by its means." This (second) chapter continues with an account of the functions of machines, the conversion of motion of a mass into heat, and the nature of the motion called heat. The next chapter discusses the various kinds of energy; gravity, elastic forces, cohesion, chemical affinity, electricity, magnetism. The classification of elastic forces among the energies is unusual, and we have always regarded elasticity as a function of cohesion, not as due to any separate and distinctive force. Prof. Stewart speaks of the "force of elasticity," but is it not rather a *property* belonging to certain bodies under certain conditions than a force? It does indeed require energy to bend a bow, but is not that energy expended in partially overcoming the cohesion of the molecules in one direction, and in approximating the molecules against their molecular motion in another? A useful condensed list of

energies is given (pp. 78-82), and this is immediately followed by the "Law of Conservation." This law asserts that the sum of all the various energies of the universe is a constant quantity, or as Prof. Stewart puts it—

$$(A) + (B) + (C) + (D) + (E) + (F) + (G) + (H) = \text{a constant quantity.}$$

Not that any single one energy is constant in itself, for they are perpetually changing into each other, but that the sum of these variable quantities is a constant quantity. The fourth chapter is entirely devoted to an account of the transmutations of energy, and the fifth gives the history of the idea from the earliest times, and then discusses at some length the dissipation of energy.

In discussing the early atomic theories, Professor Stewart has omitted the claims of Kanada, a Hindu philosopher, whose atomic theory was not only more complete and philosophical than that of Leukippos and Demokritos, but was also much earlier. A very interesting account of this theory (too little known in this country) is given by Sir John Colebrooke in his admirable articles on the Philosophy of the Hindus (in the "Proceedings of the Asiatic Society"). The statement that Demokritos "was the originator of the doctrine of atoms" is also incorrect, even as regards Greek philosophy, because Demokritos took the idea, which he indeed extended, from Leukippos. Again, the idea of the ethereal medium pervading all space was originated by the Hindus long before the time of Aristotle, and we protest against the assertion that Aristotle "caught a glimpse of the idea of a medium," when we find his constant mention of the Αἰθὴρ and remember that he introduced the very name for it which we now adopt. We claim for the ancients much more than our author is disposed to grant them: surely they possessed a very definite atomic theory, a fairly definite idea of an ethereal medium pervading space, an exact idea of the transformation of one kind of matter into another, through the intervention of some external principle of motion; and at an earlier date than all, a four-element theory, which we accept in a too literal sense, but which was philosophical and profound for those unexperimental ages. Professor Stewart next passes on to "Descartes, Newton, and Huyghens on a Medium." "In modern times, he writes Descartes, author of the vertical hypothesis . . . " (vertical should surely read vortical), but Anaxagoras of Klazomene conferred upon his νοῦς, which is essentially a mover of matter, the property of inducing the vortical motion of atoms; and of all modern philosophers, to our mind, Descartes has most drawn upon the ideas of the ancients in his Cosmogony. The historical portion of this chapter is very slight and insufficient in our opinion, but no doubt a longer historical survey would have occupied space required for other matters.

The theory of the dissipation of energy is indeed a startling one; we are perpetually converting work into heat, but it is impossible to reconvert all the heat produced into work again.

What must result? "The mechanical energy of the universe will be more and more transformed into universally diffused heat, until the universe will no longer be a fit abode for living beings." . . . . "If we could view the universe as a candle not lit, then it is perhaps conceivable to regard it as having been always in existence; but if we regard it rather as a candle that has been lit, we become absolutely certain that it cannot have been burning from eternity; and that a time will come when it will cease to burn. We are led to look to a beginning, in which the particles of matter were in a diffuse chaotic state, but endowed with the power of gravitation, and we are led to look to an end in which the whole universe will be one equally heated inert mass, and from which everything like life, or motion, or beauty, will have utterly gone away."

The final chapter discusses the position of life:—An animal is defined as a machine of a delicacy which is practically infinite, the condition or motions of which we are utterly unable to predict. And what is life? "Life is not a bully who swaggers out into the open universe, upsetting the laws of energy in all directions, but rather a consummate strategist, who, sitting in his secret chamber, before his wires, directs the movements of a great army." Prof. Stewart has surely misread a statement made by Rumford:—"It was seen that in order to do work," says our author, p. 163, "an animal must be fed; and, even at a still earlier period Count Rumford remarked that a ton of hay will be administered more economically by feeding a horse with it, and then getting work out of the horse, than by burning it as fuel in an engine." Rumford words are these:* . . . "Heat may thus be produced merely by the strength of a horse, and, in case of necessity, this heat might be used in cooking victuals. *But no circumstances could be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using the fodder necessary for the support of the horse as fuel.*" This latter must be the right view of the case, for part of the energy derived from the consumed hay is dissipated in the very working of the horse's great muscular mechanism, and a part of the work done by the horse is dissipated as heat by the friction of the working parts of the machine which it moves, whether that machine be one for producing heat by friction, as in Rumford's experiment, or for any other purpose.

Prof. Stewart's work is interesting and very readable. We commend it to all readers, whether scientific or otherwise, who are desirous of learning the most recent ideas connected with the various transmutations of the different physical forces.

* "An Enquiry Concerning the Source of the Heat which is Excited by Friction." Read before the Royal Society, Jan. 25th, 1798. The italics are our own.

Contemporary English Psychology. Translated from the French of Th. Ribot. London: Henry S. King and Co. 1873. 8vo. 328 pp.

It is always a matter of considerable interest to know the light in which our intellectual work is regarded by those who are not our own countrymen. The natural bias which causes a man or a people to view the literary or philosophical production of a fellow-countryman as unsurpassed works is not present in foreign criticism. We know how eargely M. Taine's "Notes on England," and on English Literature, were received; and although without doubt this was partly due to the eminence of the man, the cause mentioned above had also much to do with it. We have a powerful school of Psychology in this country at the present time, and a comparison and differentiation of the views of the more prominent members of it is desirable and important.

Philosophy was, in the beginning, universal science; it treated of "the universality of things, the all." Then came a separation of one of its parts, mathematics, some two centuries after the time of Pythagoras. But, according to M. Ribot, the old philosophy of Plato and Aristotle is still the universal science:—Metaphysics follow physics, politics follow morals, physiology follows psychology. In the middle ages, medicine and alchemy separated from it, and in the eighteenth century physics. Then philosophy in its broadest sense began to lose its comprehensiveness: nature was wrested from it; God and man remained to it. Philosophy once included all things, "principles and consequences, causes and facts, general truths and results;" ultimately "it will be metaphysics and nothing more." It has been said that "metaphysicians are poets who have missed their vocation," and M. Ribot considers the assertion just, but we must venture to differ from him. For surely the precise, hard, logical mode of thought which the metaphysician must adopt; his cold, lifeless theories and harsh unyielding laws ill consort with that warm flow of imagination which spontaneously should burst from the poet. Hegel's logic may indeed "border on Faust;" but what a thoroughly metaphysical poem Faust is! If all poems were like Faust, then indeed the poet and the metaphysician would have much in common. And what of the end of all philosophy? Let us suppose all our questions concerning God, nature, and ourselves, finally answered. What would remain for human intelligence to do? This solution would be its death. All enquiring and active minds will be of Lessing's opinion on this point: "There is more pleasure in coursing the hare than in catching it." Philosophy will keep up its activity by its magical and deceiving mirage. Were it never to render any other service to human intelligence than that of keeping it always on the alert, of elevating it above a narrow dogmatism, by showing it that mysterious beyond which surrounds and presses upon it in every science, philosophy would do enough for it."

M. Ribot considers at considerable length the proper definition of the Science of Psychology (a word first introduced by Goclenius). He shows that, during the seventeenth century, the science of the soul was called metaphysics; and that, hence, metaphysics and psychology have many points of connection. In its widest sense, he considers that psychology embraces "all the phenomena of mind in all animals," or if we follow Mr. Stuart Mill, and make more exact divisions, we have *General Psychology*: the study of the phenomena of consciousness, sensation, thought, emotions, relations, &c., considered under their most general aspects. This embraces *Comparative Psychology*, and *Psychological Teratology*, or a study of anomalies and monstrosities. At the conclusion of his most interesting introduction, our author tells us that since the time of Hobbes and Locke, England has done most to forward Psychology.

Then follows the main part of the book: a condensation of the psychological systems of Hartley, James Mill, Herbert Spencer, Alexander Bain, G. H. Lewes, Samuel Bailey, and John Stuart Mill. Thus the survey extends over about a century, but is mainly confined to the last thirty years.

The agreement of these philosophers in regard to all main points in each other's systems is clearly shown. These main points may be briefly stated as follows:—(*a*). Psychology examines the facts of consciousness, and connects those facts by definite laws. (*b*). It deals with phenomena, not knowing the nature of the soul or mind. (*c*). It studies these phenomena (1) objectively by signs and actions which interpret them, and (2) subjectively by memory and reason. Consciousness consists of "a continuous current of sensations, ideas, volitions, feelings," &c.; it is made up of the perception of a difference, and the perception of a resemblance. Perceptions are internal conditions corresponding to external conditions. These and many other definitions are given in the concluding remarks, in a clear, crisp form, very readable and understandable, and quite divested of unnecessary technicalities. We feel assured that the work will be received with open arms by the largely increasing school of English psychologists.

Animal Locomotion; or Walking, Swimming, and Flying. With a Dissertation on Aëronautics. By J. Bell Pettigrew, M.D., F.R.S., F.R.S.E., Pathologist to the Royal Infirmary of Edinburgh. London: Henry S. King and Co. 1873. Crown 8vo. Illustrated. 264 pp.

This work belongs to the "International Scientific Series," five or six volumes of which, including Professor Stewart's "Conservation of Energy," have already appeared, and many more are announced. The object of the author is to explain various

difficult problems in animal mechanics, and to discuss some of his views concerning the possibility of flying. A somewhat long introduction treats of the various motions possible to different creatures. It is herein shown that walking, swimming, and flying are only modifications of each other. "Walking merges into swimming, and swimming into flying, by insensible gradations," and the various differences in the actions are due to the fact that the media of support differ in density, earth for walking, water for swimming, air for flying. The relation of these actions is well shown by the fact that birds and insects can perform them all, while a large number of creatures can both walk and swim. "The subject of flight has never until quite recently been investigated systematically or rationally, and as a result, very little is known of the laws which regulate it. If these laws were understood, and we were in possession of trustworthy data for our guidance in devising artificial pinions, the formidable Gordian knot of flight there is reason to believe could be readily untied." Thus early in the book (page 4) does our author introduce an evidently pet idea that artificial flight is a possibility. The introduction is continued with various discussions of the operations performed during walking, swimming, and flying; thus we are told that the extremities of animals act as pendulums during walking, and describe curves like a figure of 8, as also do the bodies of fish in swimming, and the wings of birds in flying. Dr. Pettigrew, indeed, claims the discovery of the figure-of-8 theory. In the succeeding part of the work this theory is applied to, and illustrated by, the progression of various birds, beasts, and fishes. Some beautiful original drawings are given to illustrate these motions; we may notice particularly those which illustrate the movements of the wings of the wasp and fly (pp. 139—141).

Passing on to the subject of aëronautics, our author shows the extreme difficulty of the problem of artificial flight to consist, among other things, in "(3rd) the great rapidity with which wings, especially insect wings, are made to vibrate, and the *difficulty* experienced in analysing their movements; (4th) the great weight of all flying things when compared with a corresponding volume of air; and (5th) the discovery of the balloon, which has retarded the science of aërostation, by misleading men's minds, and causing them to look for a solution of the problem by the aid of a machine lighter than the air and which has no analogue in nature." The flightists may now be divided into two classes:—1st, those who advocate the use of balloons; 2ndly, those who consider that weight greater than the air is essential. This second class has two divisions:—"(*a*) those who advocate the employment of rigid inclined planes driven forward in a straight line, or revolving planes (aërial screws); and (*b*) such as trust for elevation and propulsion to the vertical flapping of wings." Dr. Pettigrew's work, although it may not

appeal to a large number of readers, is very readable, and will certainly be a great boon to the members of the Aëronautical Society.

Fruits and Farinacea; the Proper Food of Man. By the late JOHN SMITH, of Malton. Manchester: John Heywood; London: F. Pitman. 1873. 112 pp. Crown 8vo.

THIS small work, which has been edited by Professor William Newman for the Vegetarian Society, contains the substance of a work bearing the same title first published in 1845. It is an essay on vegetarianism, and endeavours to prove that vegetables were the original, and are the natural and best food of man. That such diet was originally adopted by mankind the author tries to show by various quotations from Genesis and other early writings. In the second chapter proofs are derived from our organs; the teeth are said to be unsuited to the mastication of animal food; and the argument is forwarded by the alleged inappropriateness of our salivary glands, alimentary canal, stomach, colon, cæcum, and liver. We really begin to wonder how man can have lived for centuries on an omnivorous diet if all his organs are unsuitable for anything but a vegetable diet. A curious table is given in the third chapter, showing the various times which different substances take to digest, after Dr. Beaumont. The amount in each case is not mentioned,—presumably equal weights; if so, it is not to be wondered at that soft boiled rice should digest in one-third the time of beef or mutton. Surely the composition of the various kinds of food should be given, in order to enable one to form a just estimate. We select a few substances:—

	Hours.	Mins.
Rice, boiled soft	1	0
Tapioca, stale bread, milk	2	0
Apple dumpling	3	0
Potatoes and turnips boiled, butter	3	30
Venison	1	35
Turkey	2	30
Boiled pork, hard-boiled eggs	3	30
Salt pork, boiled	4	30
Veal, roasted	5	30

This table is surely no great support to our author's argument, when we find that venison takes a shorter time to digest than tapioca, bread, cabbage, and milk, while boiled potatoes and turnips take longer than beef and mutton, and actually as long as that notoriously indigestible substance, boiled pork. Among other things, an attempt is made to prove that vegetable diet is "favourable to the moral state;" the use of wine is said to follow the stimulus of animal food. Noah drank wine and became

drunken soon after receiving permission to eat animal food; Jacob, when he brought his father the savoury mess of pottage, likewise brought wine. "Again," says Mr. Smith, "carnivorous animals are ferocious; the herbivorous are gentle, sociable, and playful." But, we would ask, can anything be more "sociable and playful" than a dog or a cat, anything more ferocious than a wild bull? Those who have seen wild horses fight in the prairies will class the horse with the bull. We cannot discuss such arguments as these; a similar mode of reasoning to that employed by Mr. Smith could be made to prove that black is white. This book appeals to a small class of persons; that it will convince anyone that vegetarianism is better than our present system we confidently doubt. We have been unable to find a trace of sound logic or convincing argument in the whole book, and are more than ever assured that our omnivorous diet is the right one.

Geology. By ARCHIBALD GEIKIE, LL.D., F.R.S., Director of the Geological Survey of Scotland. Illustrated. London: Macmillan and Co. 1874. 18mo. 130 pp.

THIS small volume forms the fifth of Messrs. Macmillan's Science Primers, and immediately follows the Physical Geography Primer, by the same author. The first of the series, by Professor Huxley, has not yet appeared, and is somewhat eagerly expected, as it forms an introduction to the whole series, and will without doubt discuss the modes and advantages of elementary science teaching. The author of the book before us is well known as an eminent geologist, and there is nothing to be said about his book in the way of criticism. The arrangement is clear and good; the subject-matter treats of sedimentary rocks, igneous rocks, and "organic rocks," that is, rocks consisting of the remains of plants and animals,—such as coal, chalk, and encrinitic limestone. At the conclusion we have various paragraphs relating to the crust of the earth considered as a whole, to prove that it has been upheaved and depressed at different epochs, which actions have produced tilting, crumpling, and breaking of the crust. We find here, too, a section on the origin of mountains. A few good illustrations help the beginner to realise the various descriptions.

A Phrenologist Amongst the Todas, or the Study of a Primitive Tribe in South India; History, Character, Customs, Religion, Infanticide, Polyandry, Language. By WILLIAM MARSHALL, Lieutenant-Colonel of Her Majesty's Bengal Staff Corps. London: Longmans, Green, and Co. 1873.

THE native inhabitants of the Indian peninsula may be broadly divided into two great races,—the Aryans, inhabiting the whole

of the northern and western plains,—the Dravidians, occupying the central and southern districts. The first comprehend all the more civilised peoples—the Mahomedans and Hindoos; the second are in various lower stages of civilisation, and are mostly pagans. Besides these there are a number of obscure tribes, such as the Kols, whose language is allied to some dialects of Pegu, and the Pariahs and other servile castes, who may probably represent a remnant of some of the earliest savage inhabitants of the country. The Aryans belong to the great Aryan or Indo-Germanic race, and their language no less than their physical features allies them to the highest European peoples. The Dravidians, on the other hand, are more related to the Mongolian races; and their nearest affinity has been traced by language to the Finns and Lapps far in the north-western corner of Europe. Philologists look upon these Dravidians as a much older people than the Aryans. They probably once spread over a large portion of Europe and Asia, and entered India from the north and north-west, occupying the country, and exterminating, or making slaves of, the rude aborigines. At a later epoch the higher Aryan type was developed, and drove out the Dravidians from all the more fertile parts of Europe. This energetic people were the parents of the Celts in the west, of the Germans and Slavonians in mid-Europe, and of the Sanscrit-speaking Aryans in the east; and these latter entered India from the north-west, and took the place of the Dravidians in all the more fertile and accessible districts as these latter had taken the place of the older aborigines.

The Dravidians of India now form eight tribes or nations, speaking distinct though allied languages. These are the Tamil, Telugu, Kanarese, Malayâlam, Tuluva, Toda, Gônd, and Khond, the first five belonging to more civilised peoples possessing a written character, while the three last are spoken by comparatively savage tribes. The smallest of these tribes, the Todas, is confined to the plateau of the Nilagiri Mountains, and consists of about 700 individuals. They formerly practised infanticide as an institution, and the result was that they were rapidly becoming extinct; but, owing to the influence of the Indian Government, this has been discontinued for many years, and they now increase in population with tolerable rapidity. These Todas are, in many respects, one of the most interesting and peculiar tribes in the world; and every student of man is indebted to Colonel Marshall for the careful study he has made of their whole physical, mental, and moral nature, and for the well-illustrated and instructive volume in which he gives both his detailed observations and the conclusions which he draws from them.

We will endeavour to sketch in outline the most curious features of Toda life, in the hope that we may induce many of our readers to go for fuller information to the book itself.

The Todas live in very small village communities of from 20 to 30 persons. Their houses, or huts, two or three together, and sometimes all under one roof, are low and somewhat of an inverted boat-shape, with very low doors (only about three feet high), and no windows. Attached to every village is a cattle-pen, and a separate building, which comprises the dairy and the dairyman's abode. They live a purely pastoral life, and, perhaps more than any other people in the world, are absolutely dependent for their existence on one animal,—the buffalo. Though they live in a fertile land and a delightful climate, they grow no crops, and have no kind of cultivation whatever. Though their woods and hills abound in game, they neither hunt nor trap any living thing. They keep no domestic animals but the buffalo and the cat, although the populations around them possess goats, pigs, and poultry. They eat no flesh, not even that of the buffalo, of which they often have a superabundance, but live wholly on milk and butter, with rice and such other vegetable food as they obtain in exchange for their surplus dairy produce and for the young male buffalos for which they have no use. Although surrounded by strong and often quarrelsome tribes, they possess no single weapon of offence or defence,—no bow, or spear, or sword, or club. They never fight among themselves or with their neighbours. They have no sports of activity or skill. They have no manufactures, even of the simplest kind. Two men in every village are set apart for the dairy-work, leaving all the rest to lead an almost absolutely idle life! Yet they are by no means savages of a very low type. They are quiet and dignified in their manners, amiable in disposition, and very good-looking, the excellent photographs with which the book is copiously illustrated showing us intelligent and often handsome faces, in no way distinguishable from those of many of our own country people. They are courteous to strangers and to each other, and have an elaborate system of salutations and ceremonies. Their absolute dependence on the buffalo has led them to a form of religion in which this animal is the central figure. They have a sacred breed of cattle, which are distinguished by carrying bells; and hence ancient bells are sacred. The dairy is sacred. No one except the dairyman and his assistant may enter it. During the term of their office, these two men have to pass absolutely solitary and celibate lives, they and their implements being touched by no human being. The dairymen who have charge of the sacred herds are for the time being looked upon as gods. They keep in the dairy certain relics,—old cow-bells, knives, and axes,—which are in the highest degree holy, and which the dairyman, also priest, salutes with certain ceremonies every morning. The people in general also salute the rising and setting sun, and have certain vague notions of a future state.

Our author has minutely studied this curious people, and gives us interesting details of their every custom and ceremony, habit

and superstition; so that we obtain a complete picture of the entire course of their simple lives, and a considerable insight into their mental and moral nature. Perhaps with somewhat of a student's partiality for his favourite subject, he arrives at the conclusion that they are a very ancient people, and that their existing customs and mode of life have come down, almost unchanged, from a period more remote than those of any other race known to us. He has also gone with extreme minuteness into their social statistics, taking an accurate census of a number of villages, and obtaining the ages, sex, and relationships of every individual. The results are exhibited in a series of tables, from which some very curious conclusions are drawn. The primitive custom of the Todas was to kill all female children except one or two. This, of course, resulted in a superabundance of males, and hence arose the practice of polyandry, or of some women having two or more husbands, which practice continues to this day. Although infanticide of females has long entirely ceased, yet the number of males is still largely in excess. The census tables seem to show that considerably more male than female children are born; and it is very ingeniously argued that this is the necessary consequence of long-continued female infanticide. It is proved thus. The average Toda family is six children born to each woman. Now, if we take three women, the first of whom has six daughters, the second six sons, and the third three sons and three daughters, we shall be able to trace the effects of infanticide on the proportionate numbers of the two sexes. The first mother has to destroy four daughters, keeping only two. The second keeps her six sons. The third destroys two daughters, keeping three sons and one daughter. There will remain nine sons and three daughters, and these will be the parents of the next generation. But the majority of these will be derived from families which produced more males than females, and as such tendencies are hereditary, the constant preservation of a similar majority generation after generation will inevitably result in a male-producing race; and this peculiarity having been established by a long course of artificial selection, will continue to manifest itself for an unknown period after the depraved practice which gave rise to it has been abolished.

The view that close interbreeding is not *per se* injurious is strongly supported by the case of the Todas. From time immemorial they have married together in the same or adjoining villages, till they are all closely related in various degrees of cousinship; yet the people seem to be as a whole healthy and vigorous, and remarkably free from all those diseases supposed to be produced by close interbreeding. Out of a population of 196, only 2 were malformed.

Colonel Marshall is an ardent phrenologist, yet that despised science nowhere appears prominently in his book beyond the two

chapters which he devotes to it. He insists on the remarkable simplicity and uniformity in the form of the crania of savage races compared with those of civilised and highly complex nations like our own; and this agrees with the much greater uniformity in savage character, and the less frequent occurrence of men of exceptional mental peculiarities. The extreme care and minute accuracy with which all the facts relating to this interesting people have been investigated, should secure for the author the credit of impartiality in the table of the comparative development of the various phrenological organs in eighteen individuals (males and females), taken, he assures us, without bias of any kind. The comparison of the average character denoted by the table and the observed peculiarities of the race are very interesting; and we can hardly believe that so intelligent an observer and reasoner could have failed to discover the unreality of phrenology if it were so entirely destitute of truth as it is the fashion with scientific men to assume.

The skull of the Todas is extremely and universally dolichocephalic,—that is long in proportion to its width. This form is very characteristic of low types of man, unprogressive and unenergetic. The Ancient Britons, the Lapps, Fins, Siamese, and some others, are, on the other hand, highly brachycephalic, that is, the skull is short, and approaches the globular form. Now Colonel Marshall gives us a suggestive theory of the meaning of these marked differences of form, and we believe it is the first theory of the kind that has been advanced; for, while strenuously opposing phrenology, the craniologists of the present day have not made the slightest approach to a correlation of cranial form with national or individual character. Enormous collections of skulls have been made; they have been figured and measured with the most scrupulous accuracy; the various proportions of the different dimensions have been compared; the averages of different races have been taken,—and all with no result whatever! It is, indeed, pretty generally agreed that the higher and more civilised races have the larger brains, and therefore the larger crania; but as to why these crania should differ in form and proportion so widely as they do, we obtain no light whatever. Nor has the study of the anatomy of the brain led to any more definite results; and even the recent experiments of Professor Ferrier are capable of various interpretations, since, while the phrenologists see in them a confirmation of their own doctrines, Dr. Carpenter maintains that they support his view, which is that the higher intellectual faculties are situated in the back of the head, while the development of the forehead only indicates the predominance of faculties common to man and the lower animals!

According to modern phrenology, the group of organs situated at the sides of the head, and which thus give it breadth and fulness, are termed invigorating, being those which give the

desire for conflict; for overcoming obstacles; for the acquisition of property; for animal food, and for elaborate dwellings, tools, and weapons, and which also incite to cunning and treachery. Tribes which have these faculties in excess will, as a rule, be warlike, cruel, treacherous, thievish, ingenious in constructing their weapons, huts, and canoes; fond of animal food, and good hunters, and sometimes cannibals. Tribes in which they are markedly deficient, will generally be the reverse of all this, peaceful, mild, open, honest, idle, careless in their food, huts, and clothing, and with few tools and weapons. Of course the proportions of the various organs may vary indefinitely in either form of cranium, and thus some one or other of these characteristics may be wanting; but, making allowance for this, we see a marked difference between such narrow-headed races as the low Australians, and the much higher broad-headed Sandwich Islanders, or the mild, narrow-headed Esquimaux, and the very broad-headed, warlike Araucanians. Colonel Marshall believes that the broad-headed type has been developed in the struggle for existence, and has in most cases driven out the narrow-headed where the two have come into contact. It must be remembered, however, that either extreme is an inferior type, and that it is the well-balanced organisation, with a brain intermediate in form, if sufficiently large, that will progress most in civilisation, and will be able to rule and conquer, or exterminate either extreme type. The objection, therefore, brought by Mr. E. B. Tylor ("Nature," Dec. 11, 1873) against our author's view, that the comparatively narrow-headed Russians have subjugated many broader-headed Asiatic tribes, is not to the point. The Russians are more civilised, in a higher state of organisation and discipline; and it is this, not their individual energies, that has now rendered them superior to those Eastern hordes which, at an earlier period, when their state of civilisation was more equal, would probably have overcome them. The warlike and ingenious Arabs, Afghans, and Malays, are all markedly brachycephalic. Dr. J. B. Davis states that his extensive collection of crania shows that, in most cases, the female skull is more dolichocephalic than the male, but that among many of the African races the reverse is the case. This has an interesting bearing on the fact, so strongly insisted on by our reporters of the Ashantee war, that the African women are much more industrious and energetic than the men; while we know that in Africa alone there exists an effective female army. These are suggestive facts; and it is to be hoped that they will induce phrenologists to utilise our large collections of the crania of various races, for the purpose of working out in detail the peculiarities of national character as indicated by them. This should be done by actual calliper measurements of every organ where practicable, so as to introduce precision into the results. The author of the present volume could do no better service to

his favourite study than by applying himself to this work when he returns to Europe; and we hope he may be induced thus to bring prominently before the scientific world a study which the present writer believes to be founded inductively on a wide basis of observed facts, and calculated to be of immense importance in elucidating the mental nature of man, both individually and nationally.

In this short notice we have not been able even to allude to many of the interesting topics discussed by our author. His beautifully illustrated volume should be read by every one who desires to see how much valuable matter a man of genius may obtain during a few months' sick leave; and how much light may be thrown on curious social problems, and on the early history of mankind, by the careful study of a few scattered families of an almost extinct tribe of savages.

The Ocean: Its Tides and Currents, and their Causes. By WILLIAM LEIGHTON JORDAN. Longmans, Green, and Co., 1873.

THIS is a most vexatious book. We cannot help admiring the industry with which the author has studied his special subject of ocean currents, and to a certain degree the originality of his method of treating it, but in the midst of this we are thrown back by the extravagance of originality and error, in the author's conceptions of the fundamental properties of matter, and the laws of motion. His great stumbling block is the pre-Newtonian idea that the *vis inertiæ* of matter is a continual striving for rest, or an internal resistance to the continuance of motion. He endows inertia with positive activity, and makes that activity all one sided, by representing it as an inherent property or force of matter which opposes all external force sproducing motion, but offers no opposition to the external forces which resist or destroy motion, or, to use his own words, "I have shown that *vis inertiæ* opposes motion in everything, and that its own inherent property of *vis inertiæ* must tend to bring a body to rest under any circumstances whatever, just as much, and in the same manner, as the action of any force from without; and, secondly, I have shown that, as regards the motions of the planets in their orbits, the centrifugal force which opposes the centripetal force of the bodies which compose the solar system one towards another, and all towards their common centre of gravity, is the force of astral gravitation opposing that of solar gravitation; so that in their courses they are borne smoothly along the lines of equlibrium lying between opposing forces of gravitation."

Mr. Jordan evidently imagines that the quantity of matter composing the stars compensates for their distance, and thus enables them to control the gravitation of the sun upon the planetary members of the solar system. This utter miscon-

ception of the quantitative force of gravitation due to its necessary inverse variation with the square of the distance vitiates completely all the reasoning based upon it.

A further confusion is introduced by another paradox, which appears to be of Mr. Jordan's own invention, *viz.*, "the motive force termed *evanescence*." We are told that it implies a motion of the evanescing particles, and gravitation tending to cause contraction necessitates a motion of the remaining particles; and since contraction is a necessary consequence of evanescence, having effect wherever evanescence occurs, it must act towards every point from which evanescence acts, and thus divide the universe into separate masses, and also that "certain forces are in play, causing constant change of form and place; and to the combined action of these forces (to which no name has hitherto been given) I have applied the term evanescence."

In spite of these explanations and a good deal more of similar disquisition, we confess our inability to understand evanescence.

When, however, Mr. Jordan fairly plunges into his proper subject, the Ocean, and deals with the phenomena of its currents apart from his paradoxical notions of "inertia," &c., he displays an amount of careful study and research that render his errors concerning fundamental physical laws the more vexatious. Some of his critical discussions of the theories of others are as remarkable for their clearness as his exposition of his own theories are for their obscurity. The following passage is an example.

"It seems to me that the manner in which gravitation would tend to restore disturbance of the normal level, such as those indicated by Dr. Carpenter, would naturally be by tidal movements, or easy, imperceptible movements of the whole mass of water intervening between the higher and lower levels, the former sinking and the latter rising simultaneously, just as when a trough half full of water is tilted on one side, the level of the water is maintained, not by the surface water streaming over the lower side, but, excepting the effects of friction against the sides and bottom of the trough, by an equable motion of the whole mass of water."

"When a narrow channel lies between the higher and lower level, as in the case of the Mediterranean and Baltic, this movement would form a rush of water through the Strait, but not an upper rather than an under current. It so happens that, in both these cases, so elaborately considered by Dr. Carpenter, the higher level is also the lighter water; and, therefore, by Captain Maury's simple theory, the specific gravity is restored, or an increase of the difference prevented by the heavier water running as an under current towards the lighter, and the lighter as an upper current towards the heavier. Suppose that instead of the lighter water being at the higher level, as in these two cases, the heavier were so, will any one contend that the level would be restored by a surface current from the higher to the

lower level? Is it not evident that the sweeping assertion that differences of level will be restored by surface currents is a mistake? The course of currents, as the level is being restored, clearly depends upon the relative specific gravity of the water at the different levels. The heavier water will form the under current, and the lighter the upper, regardless as to which may be at the higher or lower level; and, if there be no difference of specific gravity, the level will be restored by a tidal movement forming a current through any narrow channel intervening, but not an upper rather than an under current. The systems of upper and under currents through the Straits of Gibraltar and the Sound are clearly the result of differences in specific gravity, as explained by Captain Maury's theory, which had better be left in its original simplicity, for this modification with which Dr. Carpenter has attempted to encumber it is not an improvement, but an erroneous complication."

Here we have the vulnerable point of Dr. Carpenter's modified resuscitation of the old theory of oceanic circulation clearly indicated, and a home-thrust of clear, sound reasoning fairly delivered through it. As this point is the very heart of Dr. Carpenter's contribution to the subject, the thrust is fatal. It is followed by further and equally clear and able discussion of the details of Dr. Carpenter's arguments, and of the theories of Maury, Rennell, Herschel, &c. This chapter xx. of Mr. Jordan's book is really excellent, and worthy of careful reading, but we fear that it will not receive the attention it deserves, on account of the bad impression or prejudice which the author's fundamental physical errors must induce.

Cosmical theories are always dangerous, and their proprietors are always subject to a greater or lesser amount of martyrdom. Even if the theory is sound, its owner must die in order to obtain its acceptance; and if it is wrong, as usually happens, it is liable to insinuate itself amidst all his sounder thoughts and positive researches, and poison or intoxicate his common sense itself. Even if he has the exceptional strength of mind necessary for keeping his great theory from this sort of usurpation, he is a suspected visionary, and his most sober speculations are unnoticed." We fear that Mr. Jordan will not escape these perils.

Outlines of Natural History for Beginners. Being Descriptions of a Progressive Series of Zoological Types. By H. ALLEYNE NICHOLSON, M.D., D.Sc., M.A., PhD., F.R.S.E., &c. London: Blackwood and Sons.

THIS is a clearly-written, unpretending sketch of the subject which it treats, and well adapted to its object, viz., to assist the teaching of zoology in schools. The mode of presenting the

subject differs somewhat from that which is commonly adopted. Instead of at first presenting a general view of the animal kingdom, and its greater divisions into sub-kingdoms, Dr. Nicholson at once describes the individual types representative of the smaller sub-divisions or classes, commencing with the Aniseba as typical of the class Rhizopoda, and so on up to the dog as the representative of the Mammalia. The sub-kingdoms are afterwards described in the concluding chapter. The types are well-selected, and their characteristic features plainly described, though, of course, only in outline, the reproductive organs and the more minute details of internal structure not being described, the attention of the pupil being directed to the conspicuous points of structure, more especially to those which are external.

It is doubtless more in accordance with strict philosophy to take details first, and then to pass from them to the broader generalisation ; but, in teaching, this method is attended with some disadvantages. We think that, on the whole, it would have been better in a work of this kind to have commenced at first with an outline of the most general kind, and then to have filled it up with the matter which forms the bulk of this little work. The relations of the different classes to each other would have been better understood after the pupil had been made acquainted with the general characteristics and the broader distinctions between the sub-kingdoms. As it stands, the step by which he is carried from the Cephalopoda to the fishes is made no broader than that by which he passes from the Crustacea to the Arachnida. In a work of this kind, the greatest difficulty is to determine what should be omitted ; and opinions must necessarily vary rather widely on this point. As an expression of our own opinion, we think that Dr. Nicholson might advantageously have told the school-boy and girl just a little about the predecessors of the present generations of animal life ; for instance, when he tells them of the rarity of the Brachiopoda, and his reasons for selecting an Australian representative of this group, he might have referred to the comparative abundance of their fossil remains. Thus some of the ancient links between the fishes, reptiles, and birds might have been described just sufficiently to awaken the curiosity of the pupil and indicate the bearings of his present study upon that of the ancient history of our globe.

We are rather inclined to quarrel with the title of the book, as it sanctions and maintains the very narrow, popular fallacy of regarding zoology as the whole of natural history. "Outlines of Zoology" would have been a sounder and more expressive title.

Manual of Lunacy; a Handbook Relating to the Legal Care and Treatment of the Insane. By Lyttleton S. Winslow, M.B., &c. With a preface by Forbes Winslow, M.D., &c. London: Smith, Elder, and Co.

Not being learned in the law, it is with diffidence that we express an opinion upon the work before us. Still a careful perusal leads us to conclude that the author is thoroughly master of his subject, and that his book will be of great value to all those members of the medical profession who devote themselves to the care of the insane, or who are called upon to give evidence on the mental condition of persons accused of crime. The section on "Medical Evidence in Court" contains advice which may be useful to other scientific men as well as to physicians when they have the misfortune to appear as witnesses in a court of justice. The unfair stratagems of counsel in dealing with technical evidence are alluded to in a manner which some, doubtless, among our readers will be able to appreciate from personal experience.

The statistics on the varying amount of insanity and idiocy in different countries give wide scope for speculative inquiry. Why, for instance, should an Englishman or an Irishman be nearly five times more liable to insanity than an Austrian? Why, in the little German state of Oldenburg, should 1 in every 301 of the gross population be insane, whilst in Saxony the ratio is only 1 in 1427? Neither race, nor government, nor religion seems to offer any clue to such a discrepancy as the latter.

Introduction to the Study of Organic Chemistry. By H. E. Armstrong, Ph.D., &c. London: Longmans and Co.

This volume belongs to the useful series of "Text-Books of Science" which Messrs. Longmans are at present bringing out. We cannot say that the work displays any very striking characteristics either for good or evil. The author, having first explained organic chemistry as the chemistry of carbon compounds, touches briefly upon formulæ, empirical and rational, and upon the action of various reagents upon the carbon compounds. He then passes on to the family of hydrocarbons, and considers the remaining groups of carbon compounds in their relation to the hydrocarbons. Those who take up this volume as a book of reference, and search in it for information concerning animal and vegetable substances of importance in nature or in the arts will find themselves disappointed. But to furnish such information is certainly no part of the author's plan. His object has been to describe only those compounds whose "relations to other well-understood bodies have been satisfactorily established." The work is in its nature systematic, and substances whose constitution and relations are unascer-

tained must, therefore, be left provisionally unnoticed. The more chemistry succeeds in developing itself as a primary and exact science, the more it must, of necessity, abandon the merely descriptive and concrete features which marked its earlier days. That by so doing it must lose a certain portion of its attractions, and appeal to a different class of minds, is undeniable. To the student prepared to accept these changes we believe that Dr. Armstrong's work will be of value.

The Birth of Chemistry. (Nature Series). By G. F. RODWELL, F.R.A.S., &c. London: Macmillan and Co.

Is a knowledge of the history of chemistry necessary? No, and yes! It is quite conceivable, for instance, that a man without any knowledge of the origin and early development of the science might be the most brilliant and successful experimenter,—the most accurate analyst the world has produced. Nor can it be contended that the practical applications of the science are in the smallest degree promoted by an acquaintance with its rise and progress. But it is difficult to comprehend the philosophy of chemistry, or to view it in its relation to other sciences, without an acquaintance with the rise, the reign, and the decay of the successive theories which have prevailed in past days. As a branch of culture and a means of intellectual discipline, the history of science may justly claim a high rank. It is with this view, evidently, that the author approaches his subject. "I have endeavoured," says he, in his preface, "to trace the rise and early development of a very old science, mainly that we may mark the attitude of thought which actuated the scientific mind in bygone times, and may thus be led to compare the ancient with the modern method of evolving ideas and building them up into a connected whole." The history of science is to be studied as a basis for the methodology of science. The author likens the development of chemistry to the erection of a house. "The time when the foundation-stone was laid is too remote to be even suggested; the basis of the edifice is sunk deep in Eastern soil; the walls were slowly and laboriously raised during the middle ages, and were completed by Lavoisier, Black, and Priestley." The similitude is, in one important sense, misleading. Successive generations of experimentalists and inquirers do not merely add to the works of their predecessors. As Mr. Rodwell has elsewhere shown, they pull down and rebuild; they modify, they transform. And ever as they execute such changes, they dream that their own work is final and unchangeable. The world has, not for the first time, had its "modern" chemical views taught from every professorial chair, and eagerly imbibed by crowds of students. But before half a century had passed, the "modern" had become obsolete, and had been swept into

the limbo of forgottenness. Mr. Rodwell pursues his subject no further than to the dawn of modern pneumatic chemistry,—the epoch of Hales and Boerhaave, Lemery and Mayow. He has produced a thoughtful, suggestive, and decidedly readable book, which we hope will be duly appreciated.

Elements of Chemistry, Theoretical and Practical. By W. A. Miller, M.D., LL.D. Revised by Herbert McLeod, F.C.S. Fifth edition, with additions. London: Longmans, Green, and Co.

"Look for it in Miller" we have repeatedly heard said, when some rather recondite piece of chemical information had been vainly sought for in more voluminous works, or in files of scientific periodicals. And very often in "Miller" the fact required was found. Hence it is no wonder that the "Elements" have gradually secured that place in the favour of students which, *consule Planco*, was held by Graham and Turner. That a new edition was required is, therefore, perfectly natural.

The revision which the work has undergone does not consist merely in the insertion of the most important novelties discovered since the appearance of the fourth edition, but in a re-arrangement of the non-metallic elements which the editor conceives will "facilitate the progress of the student in the theoretical part of the science."

Many of the compounds of carbon have been removed to an appendix, as also the section on gas-analysis. An account has also been given of the most recent researches in thermo-chemistry,—a subject which is attracting the attention of some of the ablest chemists of the day, and which promises to play an important part in the progress of the science.

The Preparation and Mounting of Microscopic Objects. By Thomas Davies. Second Edition. Edited by John Matthews, M.D., F.R.M.S. London: Robert Hardwicke. 1873.

The former edition of this valuable little work has long been known to microscopists for the eminently practical manner in which its special subjects are treated, making it one of the most useful aids to the student commencing microscopical work.

The new edition will by no means disappoint its readers, the added matter amounting to fifty-eight pages. It is much to be regretted that the state of the author's health has prevented his personal superintendence of the work, but he has an able editor in Dr. Matthews, in whose hands he placed his copious store of

material. The manuscript was also submitted to Mr. T. Charters White, late secretary of the Quekett Microscopical Club, who made several valuable additions.

The book commences with an introductory chapter by the editor, containing an account derived from various sources of reagents used in histological inquiries; this will be of especial value to those commencing the study of minute anatomy. The original beginning of the work now forms chapter ii., which, under the title of "Apparatus," treats upon the materials used in mounting objects, such as glass slides, covers, and the other necessaries of the microscopist's work-table; it is here, as elsewhere, evident that the writer has seen and done everything that he describes.

The chapter on "Dry Mounting," in addition to what might fairly be expected on the subject, gives a somewhat detailed account of such objects as require special treatment before mounting in this manner, such as the collection, preparation, and cleaning of Diatomaceæ the mode of treating deep-sea soundings, both of which subjects are very fully treated, and much other valuable information.

In the portion devoted to "Balsam Mounting" some useful hints are given respecting the preparation of crystals.

The much disputed matter of "Fluid Mounting" is very fully dealt with, the author not only giving his own experience but also that of many other eminent microscopists. The chapter on dissection will prove useful to those interested in the subject, and to the same class of students the very full account of the processes of injecting and staining tissues will be welcome.

Half-Hours with the Microscope. Being a Popular Guide to the Use of the Microscope as a means of Amusement and Instruction. By Edwin Lankester, M.D. Illustrated from Nature by Tuffen West. New edition, with a chapter on the Polariscope, by F. Kitton. London: Robert Hardwicke. 1873.

The principal new feature in this edition is the chapter on the Polariscope, a subject generally avoided by writers on the microscope; and as a natural consequence this valuable aid to histological researches is, in too many instances, looked upon as a mere toy: the few pages by Mr. Kitton will be a great help to those needing information respecting the use of their polarising apparatus. The "half-hours" are, "On the Structure of the Microscope," "With the Microscope in the Garden," "In the Country," "At the Pond Side," "At the Sea-Side," "Indoors," and "On Polarised Light." The work will be, as was the former edition, of great service in guiding young microscopists as to where they are to look for employment for

their instruments; those who are without friends of kindred pursuits, or have not access to any of the now numerous microscopical clubs, will find Dr. Lankester a useful guide, and it will be their own fault if they do not learn something.

With regard to the illustrations, the artist is so well known that comment is needless.

Evenings at the Microscope; or Researches among the Minuter Organs and Forms of Animal Life. By PHILIP HENRY GOSSE, F.R.S. New Edition, 1874. Society for Promoting Christian Knowledge.

ALL who know the works of this most agreeable writer will be pleased to find that a second edition of this well-known work has been required; Mr. Gosse writes as those only can who have examined for themselves the objects they are describing. The greater part of the illustrations are from drawings on wood by the author, and it is to be regretted that so little pains should have been taken in printing to do them justice.

The descriptions of the various objects are interspersed with the needful directions as to magnifying power and apparatus employed, as well as numerous hints for collecting; this is a matter too generally neglected in describing the results of microscopical observations: microscopists who are expert themselves are apt to suppose that everyone knows as a matter of course how to follow the author, who has probably in many cases arrived at his conclusions by some peculiar mode of operation. All praise is due to Mr. Gosse for his consideration of the wants of young students.

In describing the structure of wool, the author still adheres to the old theory that the felting qualities of wool are dependent upon the serrations, instead of the undulations or waves which are very evident in the finer kinds of wool, merino for instance.* If hair was of good felting quality in proportion to its superficial roughness, surely the bat's hairs with their whorls of scales, and the branched hairs of insects, would be the best of all materials for the manufacture of felt; such is, however, not the case.

The *Rotiferæ* are very fully described, and many interesting points of their structure and life-history elucidated. This class is one which the author has made a special study, and the chapter contains a selection from his numerous papers on the subject supplemented by his more recent observations.

As might be expected, it is in the portions of the work devoted to marine animals that this eminent sea-side naturalist gives the most interesting details; here he is perfectly at home, and it is

* See paper by N. BURGESS, Trans. Quekett Microscopical Club, vol. i., p. 25, in which the subject of felting is very fully treated.

evident that the reader is under the guidance of one who is familiar with the results of shore and dredge collecting, and long and patient aquarium observations.

A considerable space is devoted to the structure of insects. In remarking on their eyes and ears, the author writes: "It is not impossible, judging from the great diversity which we find in the form and structure of these and similar organs in this immense class of beings, compared with the uniformity that prevails in the organs of sense bestowed in ourselves and other vertebrate animals, that a far wider sphere of perception is open to them than to us. Perhaps conditions that are perceptible to us only by the aid of the most delicate instruments of modern science may be perceptible to their acute faculties, and may govern their instincts and actions. Among such we may mention, conjecturally, the comparative moisture or dryness of the atmosphere, delicate changes in its temperature, in its density, the presence of gaseous exhalations, the proximity of solid bodies indicated by subtle vibrations of the air, the height above the earth at which flight is performed, measured barometrically, the various electrical conditions of the atmosphere; and perhaps many other physical qualities which cannot be classed under sight, smell, taste, or touch, and which may be altogether imperceptible, and therefore altogether inconceivable by us."

It is to be regretted that the author should have contented himself with borrowing a cut on page 151, representing the proboscis of the blow-fly, instead of supplying a figure more in accordance with recent observations drawn by himself.

The work can be safely recommended as a guide to those really in earnest as to their microscopical studies, and will prove an admirable companion to those who take their microscopes to the sea-side during the approaching summer holidays.

A Handbook of Practical Telegraphy. By R. S. Culley, Member Inst. C.E., Engineer-in-Chief of Telegraphs to the Post-Office. Adopted by the Post-Office and by the Department of Telegraphs for India. Sixth edition, revised and enlarged. London: Longmans, Green, Reader, and Dyer. 1874. 8vo., pp. 443.

It is a sure sign of the high estimation in which Mr. Culley's work is held, and of the soundness of his electrical knowledge, when we find that his volume has already reached its sixth edition. Holding a foremost position in his profession, and surrounded by the whole telegraphic network of the British Isles, he necessarily constitutes the centre towards which a vast mass of electrical and telegraphic information naturally concentrates; and, as a consequence, his writings may be considered the digest of these experiences.

We are informed in the preface that many portions of the book have been re-written,—incumbent on the author by reason of the rapid extension of all the branches of the profession. So numerous have been the alterations and additions that we do not hesitate to state that the labour of passing the book through the press must have cost Mr. Culley great time and close application; in fact, as much as would be necessary to compile the whole volume. Amongst the extra matter, we find an important and lengthy chapter on the Duplex System, which will be eagerly read. In this chapter, the author undertakes the explanation of the question from the commencement; and in so clear and popular a manner does he carry the reader along with him through the different stages that everyone who studies the subject can most easily follow him. There are various methods of telegraphing by the duplex system, three of which are well known, viz., "the Bridge," "the Differential," and "the Leakage." The two former, however, are the systems that Mr. Culley has treated of; the third he does not refer to, because, as we suppose, he does not consider it of sufficient practicability to deserve attention. Students, however, we are sure, would have been glad to have had his experience on the subject, and to have had an explanation of the *modus operandi* from his pen.

The comprehension of the work is large and varied. It includes:—

Part I., Sources of Electricity. Part II., Resistance and the Laws of the Current. Part III., Magnetism and Electro-Magnetism. Part IV., Induction. Part V., Atmospheric Electricity and Earth Currents. Part VI., Insulation. Part VII., The Construction of a Line of Telegraph. Part VIII., Ordinary Testing. Part IX., Description of Instruments. Part X., Submarine and Underground Wires. Addenda of various useful tables of reference.

The same determination has guided Mr. Culley in this as in former editions; he has excluded from his work all theories based upon unproved hypotheses, and has thereby maintained its character as a "practical handbook" on all matters relating to the science of telegraphy. As a handbook it is unexceptional, and is rendered far more readable from the profuse illustrations interspersing the book at almost every page. Some idea may be formed of its rich endowment in this respect, when we mention that it contains over a hundred and fifty engravings, exclusive of nine full page illustrations, and six large folding plates of very useful designs. With such a work in his hands, we believe every operator on our lines, however backward he may be in his electrical knowledge, would soon acquire a sound acquaintance with the practice of the science, and to them, as well as to general scientific students, we heartily recommend the work.

Student's Class Book of Animal Physiology. By T. AUSTIN BULLOCK, LL.D. London: Relfe Brothers.

THE author tells us in his preface that "This book, whatever be its fate, has come of a careful and earnest endeavour to meet the requirements of higher and middle class schools, and of all teachers who may desire to prepare pupils for Government or other science examinations."

We quite believe in the care and earnestness of the manufacture of this book. Every page displays the result of careful and earnest cramming on the part of the author himself, and an equally careful and earnest effort to transfer the cram to his readers or pupils. The machinery by which he seeks to effect this is a series of questions followed by mechanical answers suitable for the requirements of those michievous people who call themselves teachers, and whose teaching consists in putting certain books in the hands of their unfortunate pupils, marking so many lines to be "got off by heart," and then listening to their misguided victims while they "say their lessons."

The following is a sample:—

Q. What are the names and positions of the interior bodies of the brain?"

"*A.* From the base upwards we have (1.) The Posterior and Anterior Pyramids, and quite close by the side of the latter are (2.) The *Olivary Bodies*, with the *Restiform Bodies* on their outer sides; (3.) The *Pons Varolii*, a mass of neurine an inch wide, with its fibres running like a bridge across the Medulla Ob.; (4.) The *Corpus Dentatum*, a mass of grey neurine with a tooth-shaped edge, in the anterior of that part of the cerebellum at the sides of the Pons; (5.) The *Crura Cerebri* (legs of the brain) rounded masses of nerve fibres, which, from the Medulla Ob., emerge at the upper end of the Pons; (6.) The *Corpora Albicantia*, two pear-shaped silvery bodies at the base of the brain, but connected with the cerebrum; (7.) The *Corpus Callosum* which unites the two hemispheres of the cerebrum; (8.) The *Pituitary Bodies*, between the *Olfactory Nerves;* and (9,) The *Tuber Cinereum* just under the latter (Fig. 25, V. between V and W; S.E.P.M.; Corpus Callosum between I. and G.; I. K).

Fig. 25, to which these utterly unintelligible references are made, is a representation of the *exterior* of the base of the brain. The next question is—

"*Q.* What are the other bodies in the interior of the brain."

This is answered in like manner by simply enumerating the Thalami, Corpora Striata, Corpora Quadrigemina, and the Commissures in the same parrot-like manner, and again referring to Fig. 25, in which *none of these "Bodies" are shown.*

This is all the information afforded on this part of the subject. Anybody at all acquainted with the structure of the brain may imagine the state of mind of the unfortunate pupil who strives to obtain any *ideas* from such a string of mere names. What must

be his notions of the anterior and posterior pyramids, of the legs of the brain, &c.?

From this sample of anatomical exposition let us pass to one or two physiological disquisitions. The following is the answer to the question "What is the Sensorium?"

"That ideal point of the brain which the older physiologists, and some modern school books, call the *Sensorium*, and regard as the *seat* and *abode* of soul, is merely a fancy organ which has no existence; but the Sensorium of modern physiologists is a series of ganglionic centres at the base of the brain, including the *Olfactory Bulb*, or ganglion, and the auditory and optic ganglia."

The spinal cord is described as "the seat of a set of definite and combined muscular movements, which are altogether independent of sensation and volition; and it gives to the whole muscular mechanism a healthy contraction and tone, which at once disappear on its destruction."

"*Q.* What is the reason that certain kinds of animals live in the human stomach in spite of the powerful action of the gastric juice?

"*A.* On living substances the juice has no power, but it acts immediately after death, and it has been found that it sometimes dissolves and perforates the stomachs of the dead. Bone, even iron, and other metals gradually yield to its action; and there are instances of nails and clasped knives having been reduced to mere fragments in the human stomach."

Lesson 14 is headed "Gases in Blood." The first question of this lesson is:—"How is the colour of the blood affected by O and CO_2, and why is O constantly supplied and CO_2 as constantly thrown off?" This and the next question, "Why is CO_2 found in the blood, and how is it related to the temperature of the body?" are answered as though oxygen and carbonic acid gases are actually and abundantly mixed with the blood "thrown off," and circulate with it. In reply to the question, "What is the *exact* chemical composition of albuminous substances?" the pupil is told "that all these substances consist of C, H, O, N, S, and that some of them possess P in addition," and further on, that "gelatine consists of the same elements as albumen, but combined in smaller proportions." This style of actual looseness and inaccuracy, with affected technical profundity, prevails throughout, and is calculated, even where no actual blunders are made by the author, to convey most erroneous ideas to the pupil.

The author, in his preface, says, "We have dealt little in general description, or in those 'bird's-eye views' which try hard to exhibit all, succeed well in showing nothing. A really earnest student who prepares to meet remorseless examiners must see the subject not in bird's-eye view but from the plane of the facts and circumstances themselves." We pity any "earnest student"

who relies on this or any of the kindred works that are written by incompetent teachers for the mere purpose of cramming with phrases and technicalities in order to pass examinations. In the learning of languages, whether living or dead, this wretched method of packing the memory may be sufficient; but we warn all candidates for science examinations against any attempts to "get up" any branch of science by mere efforts of memory. Something higher than this is demanded of the scientific student. He must not merely learn his lesson,—he must understand his subject; for the remorseless examiner in science at once detects the ignorance of the candidate who merely answers by rote. If he is at all qualified to conduct a scientific examination, he will always include among his questions some theoretical and practical problems which ignominiously convict the crammed candidates, and he condemns their blunders far more remorselessly than the shortcomings of the conscientious student who has learned much less but understands a little more. A simple rudimentary treatise on physiology which, in the same space as the book before us, attempted to teach conscientiously and thoroughly about one-tenth of what is here pedantically heaped together, might enable a student to pass some of the more elementary examinations on physiology adapted to junior pupils; but this "Student's Class Book" is utterly worthless for the purpose of the "really earnest student," and can only serve as a delusion and a snare even to the student who is so misguided as to attempt to pass an examination in physiology by merely cramming the memory with technicalities and learned phrases.

A Treatise on Watch-Work, Past and Present. By the Rev. H. L. Nelthropp, M.A., F.S.A. London: E. and F. N. Spon. 1873.

Let no one suppose that it is the watch-maker alone who will be interested in this little treatise. Indeed we doubt whether the work will find much favour in the trade; for the reverend writer exposes so many of the malpractices of the watch-maker's craft that he will probably find himself as unpopular in Clerkenwell as Mrs. Stowe is said to have been in the Southern States after the publication of her anti-slavery novels.

During a century and a half, dating from about 1660, when Dr. Robert Hooke invented and applied the balance or pendulum-spring, the science of horology made great advances in this country. Those were the palmy days of watch-making when the workman took an intelligent interest in his work and sought to gain a reputation by the quality of his escapements. But, *tempora mutantur*, and at the present day our author maintains that there is not a rising man to lay claim to the sceptre of such makers as Mudge, Arnold, or Earnshaw, and that "in a few

years it is not probable it will be possible to find in Clerkenwell a workman capable of doing first-class work." To remedy this lamentable state of things, Mr. Nelthropp suggests that it would be desirable to establish a school in connection with South Kensington, and to revive, in some measure, the powers of the Clock-makers' Company, so that members should be elected only after examination by a competent governing body.

Although watch-making seems to be falling into this state of decay in England, there are yet many amateurs who take great interest in the art. But with the exception of Mr. Denison's excellent "Rudimentary Treatise," published some years ago in Weale's series, we do not remember any modern English work on this subject.

After defining the terms used in watch-work, and describing the tools used by the watch-maker, Mr. Nelthropp presents the reader with a sketch of the history of time-keepers. "Le ciel est une horloge constante et perpétuelle," and therefore the sun-dial was the earliest form of time-piece. As to watches, the antiquary knows that they were first made by Peter Hele, of Nuremberg, about the year 1500, and were called—from their place of manufacture and from their shape—"Nuremberg eggs."

Passing from the historical to the technical portion of the work, we find rules for calculating the number of teeth of wheels and leaves of pinions, and the necessary calculations for trains. Descriptions are then given of the five principal scapements—the verge, the horizontal, the duplex, the lever, and the detached escapement. A translation of a treatise on the pitching of wheels and pinions, by the astronomer M. de Lalande, is given in the shape of an Appendix.

Although Mr. Nelthropp's volume is not the work of a man practically engaged in the trade, it may be recommended not only to the antiquary who delves into the history of the watch-maker's craft, but also to every intelligent person who seeks to know something about the anatomy of the little time-keeper which he carries in his pocket.

PROGRESS IN SCIENCE.

MINING.

Owing to the alteration in the system of collecting the Mineral Statistics of Great Britain, introduced by the new Mines' Regulation Acts, the publication of the last volume of Mr. Robert Hunt's valuable Statistics, giving the returns for 1872, was unavoidably delayed until so late in 1873 that we were unable to notice them in the last number of this Journal. The method of obtaining voluntary returns, originally initiated and for many years successfully carried out by the present Keeper of Mining Records, has been displaced by a compulsory system, under which the returns are forwarded, through the local Mining Inspectors, to the Secretary of State for the Home Department. It may be well to point out that the working of this system, in its present form, is fraught with much inconvenience. Thus, by a curious accident in the wording of one of the clauses of the Coal Mines' Regulation Act, no one, except the Inspector and the Home Secretary, is permitted to see the returns, unless express consent be given by the coal-owner. Hence the Keeper of Mining Records himself is actually shut out, in most cases, from consulting the very returns which it is his duty to collate and present, in an aggregated form, to the public.

One great cause of delay in the publication of the last volume of "Mineral Statistics" is attributable to the fact that, although the Metalliferous Mines' Regulation Act, 1872, requires that all returns shall be made to the Inspectors by the 1st of August in each year, yet many of the returns on this first occasion were not in the Inspector's hands until late in November. Moreover, the Act requires only a return of the ores raised, and consequently their value and percentage of metal have to be obtained from other sources. Mr. Hunt has therefore made use of the returns to the Stannary Court for the ores of Devon and Cornwall, and the Public Ticketings for the copper-ores sold in Cornwall and Swansea.

Following our usual course, we here present a general summary of the quantity and value of the minerals raised in the United Kingdom during the year 1872:—

	No. of Mines.	Tons.	Cwts.	Value.
Coal	3001	123,497,316	0	£46,311,143
Iron ore	266	16,584,857	0	7,774,874
Copper ore	117	91,983	0	443,738
Tin ore	162	14,266	0	1,246,135
Lead ore	455	83,968	3	1,146,165
Zinc ore	63	18,542	12	73,951
Iron pyrites (sulphur ore)	35	65,916	3	39,470
Arsenic	15	5,171	15	17,964
Wolfram	3	88	5	993
Cobalt	1	1	0	20
Manganese	3	773	0	38,865
Fluor spar	1	80	12	40
Ochres, umbers, &c.	5	3,326	15	8,227
Bismuth ore	1	2	0	—
Barytes and chloride of barium ..	26	9,157	17	7,208
Clays, fine and fire (estimated) ..	—	1,200,000	0	450,000
Other earthy minerals (estimated)	—	—		650,000
Salt	—	1,309,497	10	654,748
Coprolites (estimated)	—	35,000	0	50,000

Total value of minerals produced in the United Kingdom in 1872 .. £58,913,541

Under the name of the Aërophore, M. Denayrouze has brought out an ingenious apparatus for furnishing to the miner a supply of fresh air in the midst of a deleterious atmosphere. The air is compressed by a double-barrelled pump of peculiar construction, the cylinders being movable, whilst the pistons are fixed, thus reversing the usual arrangement. By means of a regulator the pressure of the air may be adjusted at pleasure. The miner inhales the air through an india-rubber mouth-piece, whilst a supply is delivered on similar principles to his lamp. The Denayrouze lamp is specially constructed to burn independently of the surrounding atmosphere, and derives its air solely from the lamp-regulator. In one form of the Denayrouze apparatus the air is compressed into strong reservoirs, at a pressure of 20 atmospheres. Experiments putting the apparatus to tests which seem to be sufficiently severe have recently been conducted in this country,—it is said with much success. No doubt such an apparatus might be of special service in re-opening a pit after an explosion, and before it is expedient to admit fresh air; but it is evident that apparatus of this kind can have but a limited application, and is not likely to be used for prolonged work.

To prevent the collier from tampering with his Davy-lamp, Messrs. Bailey and Waddington have patented the application of a seal to the locked lamp. This seal is enclosed in a case, and so arranged that the miner cannot unscrew his lamp and expose the light without breaking the seal, and thus convicting himself.

At a recent meeting of the North Staffordshire Institute of Mining and Mechanical Engineers, a paper was read, by Mr. T. M. Goddard, on "Better Communication in Pit-Signalling by means of Electricity." He described the system of electrical signalling employed with much success at the Goldenhill Colliery. This system was said to be much more economical than the ordinary method of using a stranded bell-wire, and not so liable to get out of repair. Moreover, less room is required in the shaft with this system.

The subject of coal-cutting by machinery was recently brought before the Cleveland Institution of Engineers, by Mr. J. S. Jeans, of Darlington. After tracing the history of such machinery, he described two of the most popular coal-cutters,—namely, Mr. W. Firth's machine, and the Gartsherrie coal-cutter of Messrs. Baird. The latter has been successfully working at the Hetton Colliery.

"An Essay on the Prevention of Colliery Explosions," by Mr. Emerson Bainbridge, of Sheffield, has appeared in the columns of the "Colliery Guardian." After enquiring into the cause of such accidents, the author examines how far the means now used for their prevention can be considered successful, and points out what he considers should be done by legislation and by mining-engineers to prevent such explosions, and to lessen their fatal effects when they do occur. Mr. Bainbridge's experience and position should secure a respectful attention to his views. This essay is one of those written in competition for the Hermon prizes. The three prize-essays—those of Mr. W. Galloway, Mr. W. Creswick, and Mr. W. Hopton—have been published in a separate volume. Another of the competitive essays, by Mr. J. Harrison, of Eastwood, Notts, has appeared in the "Mining Journal."

An "Official Report on the Coal-Fields of Victoria," prepared by Mr. J. Mackenzie, the Government Examiner of Coal-fields in New South Wales, has been recently issued. It contains a careful description of the several coal-deposits of the Colony, illustrated by sections, and offers suggestions where search should and should not be made for payable seams of coal.

Mr. H. B. Medlicott has published, in the "Memoirs of the Geological Survey of India," some "Notes on the Narbadá or Sátpúrá Coal-Basin." It seems probable that we have in this basin a more complete representation of the great plant-bearing rock-series of India than in any other part of the Peninsula. The writer advises that the experiment of boring for coal within the field, at a distance from the actual outcrop, should be made at Budi, in the Dudhi Valley.

The peculiar conditions under which diamonds occur in the fields of South Africa have been well described by Mr. E. J. Dunn, who, before his recent return to the Cape, contributed to the Geological Society of London an interesting paper on this subject. The so-called "dry diggings," such as the well-known Colesberg Koppje and Du Toit's Pan, are circular areas surrounded by horizontal shales. As to the diamond-bearing rock itself, Mr. Dunn describes it as an eruptive mass brought up to the surface through pipes which have penetrated the shales, and turned up their edges. On sinking into these pipes, the general sequence of deposits is—first, a few feet of sand; then, a layer of calcareous tufa; and this deposit passes gradually into an altered igneous rock, the true character of which is still an enigma. It has been suggested that it may be a gabbro or euphotide, serving as the base of a breccia which contains fragments of shale, dolerite, and other rocks. Can this euphrotide breccia be regarded as the actual mother-rock of the South-African diamonds?

METALLURGY.

Under the head of "Mining" we publish this quarter a summary of the "Mineral Statistics" for 1872, showing the quantities of ores raised in the United Kingdom during that year. In connection with these returns, and to show the position of our metallurgical industries, we may present the following statement, which exhibits the quantities and values of the several metals obtained from the ores raised in 1872:—*

	Quantity.		Value.
Pig-iron	Tons	6,741,929	£18,540,304
Copper	,,	5,703	583,232
Tin	,,	9,560	1,459,990
Lead	,,	60,455	1,209,115
Silver	Ozs.	628,920	157,230
Zinc	Tons	5,191	118,076
Other metals (estimated) ..		—	2,500
			£22,070,447

An interesting feature is this year introduced into the "Mineral Statistics," in the shape of returns giving the consumption of coal in our blast-furnaces. These figures show that the quantity of coal consumed, on an average, for every ton of pig-iron produced, in 1872, amounted to 51 cwts. The economy of our iron-masters becomes evident on remembering that it was computed by the Royal Coal Commission that 3 tons of coal were consumed per ton of pig-iron.

Mr. T. Hughes, of the Geological Survey of India, has contributed to a recent number of the Survey "Records" some notes on the "Iron-Deposits of Chándrá in the Central Provinces," in which he gives some interesting details respecting the relative amounts of ore and fuel ordinarily employed by the natives in their furnaces. The wealth of Chándrá in iron ores is considerable, and the value of the deposits is likely to be increased by the occurrence of coal in the neighbourhood. The native furnaces, though still liliputian, are larger than those commonly in use in Bengal, and several attain a height of nearly 6 feet. The section of the furnace is that of a cone; the hearth, as usual, slopes from behind forwards; the tuyeres are each 9 inches long, 1½ inches diameter at the larger and ¾ inch at the smaller end; and the bellows are usually worked by hand. Taking the mean of several experiments, and reducing the weights to English units, we find that 8 tons of ore and 14½ tons of charcoal are consumed in the manufacture of 1 ton of wrought-iron by this method.

* Mineral Statistics of the United Kingdom of Great Britain and Ireland, for the Year 1872. With an Appendix. By Robert Hunt, F.R.S., Keeper of Mining Records. Longmans, 1873.

The Cleveland Institution of Engineers has been lately engaged in discussing Mr. Wood's methods of utilising blast-furnace slag. The slag is allowed to run from the furnace into water placed at the bottom of an iron drum rotating in a vertical plane. By this means the slag is disintegrated, and forms a powder called "slag-sand," which may be mixed with lime and used as mortar. The slag, in another of Mr. Wood's processes, is received on a flat, circular, rotating table of iron, where it is suddenly cooled, and spreads out into thin layers, which may be readily broken up and used in the preparation of concrete. Although there is perhaps no great novelty in Mr. Wood's methods, they are nevertheless likely to be of much value in using up a great deal of the Cleveland slag. In 1862 Mr. Gjers obtained a patent for running a stream of slag into water, but he allowed his patent to lapse. One great feature in Mr. Wood's machine is its economy of water. Mr. Jeremiah Head calculated that the cost of water would be only about $\frac{1}{13}$th of a penny per ton of slag disintegrated.

To determine the elasticity of metals, two different methods may be employed—either direct traction or transverse flexion. The discrepancy in the values obtained by these two methods in experiments with steel have been theoretically investigated by M. Peslin, in a paper, "Sur la Ténacité de l'Acier," published in a recent number of the "Annales des Mines."

In connection with this subject we may refer to a paper on "Tests of Steel," by Mr. A. L. Holley, read before the American Institution of Mining Engineers, in which the writer strongly condemns the practice of confining ourselves to mechanical tests, and staunchly advocates that all tested samples should be submitted to chemical analysis. In order that engineers may know what to specify, and that manufacturers may know what to make, a knowledge of the chemical composition of steel becomes absolutely necessary.

We observe that a paper "On the Molecular Changes produced in Iron by Variations of Temperature," by Prof. R. H. Thurston, of the Stevens Institute of Technology, has been reproduced in "Iron."

M. Pirsch-Baudvin has patented a new alloy, said to bear a strong resemblance to silver in many of its physical characters. A very white metal, forming a good imitation of silver, may be made of—Copper, 71; nickel, 16·5; cobalt, 1·75; tin, 2·5; iron, 1·25; and zinc, 7: about 1·5 per cent of aluminium may be added. In the preparation of this alloy certain precautions are necessary in the order and manner in which the constituents are mixed. The cobalt is said to determine many of the characters of this alloy.

The well-known "Revue Universelle des Mines, de la Métallurgie, des Travaux Publies, des Sciences, et des Arts appliqués a l'Industrie" is about to appear in an English dress. The proprietors have arranged for an English translation of each number, which will appear almost simultaneously with the French edition. This "Review" has recently contained a capital Report of the Liége Meeting of the Iron and Steel Institute.

MINERALOGY.

Corundum has been discovered, within the last two or three years, in deposits of considerable extent, and under conditions of unusual interest, at a locality, now known as Corundum Hill, in Macon County, North Carolina. Colonel Jenks, who discovered these deposits and has established workings for corundum at the Culsagee Mine, has recently visited this country, bringing with him a collection of specimens of great beauty and scientific interest. These have been submitted to the Geological Society, accompanied by notes on their mode of occurrence. It appears that the corundum is found in veins in a hill of serpentine, and is largely associated with chloritic minerals, such as ripidolite and jefferisite. Some of the crystals are notable for the intimate manner in which they are blended with these minerals, whilst others exhibit such brilliancy as to suggest that but little more is needed, in the way of purity of colour and freedom from flaws, to transfer them to the category of true gem stones. It is hardly too much to expect that sapphires, rubies, and the

other valuable forms of alumina, may be yielded by the further working of these deposits. We understand that some of the crystals have been examined microscopically by Mr. H. C. Sorby.

For several years past Dr. F. A. Genth has devoted much attention to the study of *Corundum*, especially noting the characters of the associated minerals and the changes which corundum is supposed to undergo. The results of his studies have been published in the shape of the first part of the "Contributions from the Laboratory of the University of Pennsylvania." Although corundum has been found in America both in the Laurentian system and in rocks referred to the Taconic system, yet the greater part of the American corundum occurs in what is termed the chromiferous serpentine or chrysolite formation, and in the adjacent rocks. By comparing the associated minerals and the general conditions of occurrence of the emery and corundum of Asia Minor and the Grecian Archipelago with those of the most important American deposits, Dr. Genth is led to suggest a correspondence of geological age in the two cases. The author gives a long catalogue of minerals supposed to be produced by the alteration of corundum, and refers in some instances to true pseudomorphs as evidence of such changes. There are probably few geologists, however, who will be prepared to receive Dr. Genth's theory which seeks to explain the origin of certain beds of mica-schist, chlorite-schist, and paragonite-slate, by the alteration of larger deposits of corundum. Among the minerals associated with the American corundum are four described as new species, or varieties, under the names of *Dudleyite*, *Kerrite*, *Maconite*, and *Willcoxite*.

Dr. Fischer, well known for his microscopic studies of various minerals, has examined a number of specimens of so-called *Cat's-eye*; that is to say, quartz which presents, when polished, a curious band of light generally referred to the presence of enclosed fibres of asbestos. In all the specimens examined by Fischer he could find no trace of any fibres of asbestos, so that we are compelled to seek a new explanation of the fibrous character of the cat's-eye. The quartz may either possess a true fibrous structure of its own, or it may have replaced some mineral which originally possessed such a structure. It is the latter conclusion that Dr. Fischer is disposed to accept. This conclusion is interesting in connection with Wibel's examination of the cat's-eye of the Cape, noticed in last quarter's chronicles. Fischer's paper will be found in Tschermak's "Mineralogische Mittheilungen," published in Vienna.

A new mineral-species belonging to the group of felspars has been lately described by Von Kobell, in the "Journal für Praktische Chemie," under the name of *Tschermakite*, a name proposed in honour of Dr. Tschermak, who has done so much to simplify our views of this important group of rock-forming minerals. Tschermakite occurs at Bamle, in Norway, where it is associated with Kjerulfine. The new species is found in compact masses, with very perfect cleavage in two directions, making an angle of 94°. These cleavage-planes show fine striæ, similar to those on other triclinic felspars. Analysis gives—Silica, 66·57; alumina, 15·80; magnesia, 8·00; soda, with trace of potash, 6·80; water, 2·70 = 99·87. From this analysis may be deduced the formula $3(RO.\frac{3}{2}SiO_2)+Al_2O_3.3SiO_2$. Tschermakite may therefore be regarded as a peculiar species of felspar, related to oligoclase, but containing no lime.

An excellent monograph of the minerals grouped together under the general name of *Brochantite*, has been laid before the Vienna Academy by Dr. Schrauf, and published in its "Sitzungsberichte." He describes in much detail, and illustrates by admirable figures, the several forms of this mineral, of which four distinct types are recognised. It appears that Brochantite is not prismatic in crystallisation, but that most varieties are either monoclinic or triclinic.

The peculiar form of silica which Prof. Maskelyne some time ago discovered in the Breitenbach meteorite, and described under the name of *Asmanite*, has been recently studied by Prof. Vom Rath. His conclusions confirm those of Maskelyne, respecting the specific gravity, degree of hardness, and chemical

composition. Vom Rath's analysis gives—Silica, 96·3; ferric oxide, 2; magnesia, 1·1; lime, trace. There seems, therefore, no doubt that Asmanite is a new form of silica crystallising in the rhombic system, so that we are now acquainted with three species of crystallised silica, distinguished by their specific gravity in this wise:—

Quartz	sp. gr.	2·6
Tridymite	,,	2·3
Asmanite	,,	2·24

One of the rarest known mineral-species is *Roselite*, a substance determined by Levy in 1824, and named in honour of Gustav Rose. It appears that only two specimens are known, the one in the Werner Collection at Freiberg, in Saxony, and the other in the Turner Collection in this country. Both came from Schneeberg, in Saxony. Two new specimens, recently found at this locality by Herr Tröger, have been described by Prof. Weisbach.

A continuation of Rammelsberg's researches "On the Composition of the Natural Compounds of Tantalium and Niobium" has appeared in a recent number of "Poggendorff's Annalen." The present portion of the investigation relates to the minerals called pyrochlore, yttrotantalite, fergusonite, polyclase, euxenite, wöhlerite, samarskite, and æschynite.

Prof. Zepharovich has presented to the Vienna Academy a further description of his new mineral, *Syngenite*. He now finds that the species must be referred to the monoclinic and not to the rhombic system; that the artificial salt having the same composition (namely, $CaSO_4+Na_2SO_4+H_2O$) is also monoclinic; and that Rumpfe's mineral, *Kaluszite*, is in all probability identical with syngenite.

Dr. Burkart, of Bonn, has described in Leonhard and Geinitz's "Neues Jahrbuch" the mass of meteoric iron known as the Descubridora meteorite. This was found near Poblazon, in Mexico. The same mineralogist has a paper on the occurrence of the various minerals of tellurium and bismuth in the United States.

Among recently-published papers bearing as much on physics as on mineralogy, we may cite a memoir by Dr. Carl Klein, of Heidelberg, on the optical characters of the Epidote of Sulzbach; a paper by Prof. Nöggerath, of Bonn, on the phosphorescent glow exhibited by agates and other siliceous stones when ground on the large wheels used in the polishing-mills at Oberstein and Idar; and a paper by Dr. Behrens, of Kiel, on the spectrum of the precious opal.

ENGINEERING—CIVIL AND MECHANICAL.

Boilers.—At the present day when every manufacture is carried on with the aid of steam, the strength of boilers is a most important question with reference to the duty they are expected to perform. This, as a section of experimental science, has been very much neglected in this country. From an article which appeared in the "Nautical Magazine," on the United States Experimental Commission, it appears that the majority of the experiments made by Fairbairn, on the collapsing of tubes, and upon which a rule was based for the construction of steam-boilers, were with tubes of No. 19 wire gauge, or about $\frac{1}{23}$rd of an inch in thickness. According to the practical rule based upon these experiments, the strength against collapsing is directly as the square of the thickness, and inversely as the length, but the constant multiplier for collapsing pressure is given 71 per cent above the mean of the experiments, and 25 per cent above the maximum result obtained. Before September, 1871, Mr. Francis B. Stevens, of Hoboken, New Jersey, had made several experiments on the strength and proper management of boilers belonging to the United Railroad Companies of New Jersey; and so valuable did the results appear, that the Executive Committee of the United Companies voted 10,000 dollars for the continuance of the experiments, which were made on real boilers and with steam-power. The paper from which we quote was written before the completion of the experiments; but, so far as they had gone, they fully indicated

the danger produced by constructing a boiler so that the failure of any one stay would be fatal to the whole structure; and this is the more important since stays are seldom made to take the strain equally. The importance of the proceedings of this Experimental Commission was considered so great that the subject was brought before Congress, by whom 100,000 dollars have been voted to carry out similar experiments on a larger scale, and the Commission appointed for that purpose has made a beginning, but has not yet issued its report.

According to the report of the Midland Steam-Boiler Inspection and Insurance Company for the last half of 1873, they had 3555 boilers under their care, and during that period there had been no explosion of any boiler under their inspection. In consequence of the explosion of a blast-furnace boiler, attention has been directed to the safer working of such boilers of considerable length, it is suggested that they should be divided into separate lengths connected with narrow necks or pipe connections so arranged as to prevent rigidity. The feed water, in such case, would enter the back compartment and overflow into the next one, and then again into the front compartment, supposing the boiler to be in three compartments. The last compartment alone would require a water-gauge. The water, therefore, would gradually advance from back to front, while the heat would be greatest in the front. In this arrangement, apart from the diminished danger of explosion, it will be seen that the mud would be deposited in the back end, where, the heat not being sufficient to form it into a hard scale, it could be discharged by the blow-cock.

With regard to tube boilers, it is remarked that they can evidently be worked with success where the high pressure for which they were designed is required, and where good water can be obtained, but they do not offer much advantage for low pressure, or with the average scale making water. The experience of the past year confirms the opinion that no boiler is free from the danger of explosion if not well looked after; and that the best means of preventing explosion is to insist upon frequent inspection and careful attendants.

London Water Supply.—The Kent Company are now giving a constant supply to about 2200 houses in Deptford, and is extending the constant service to other parts of its district. The New River Company have now the power of affording effective constant service in their district; and they have also commenced a new high service covered reservoir, to contain one million gallons, at Southgate, in anticipation of the requirements of the water supply to Edmonton parish. This Company has, in a number of cases, afforded constant supply by means of stand pipes, and have agreed with a Committee of the Corporation of the City of London to furnish constant supply at once to a large number of the houses of the poor within the City bounds, wherever the arrangements of the officers of the Corporation in connection therewith are completed. The East London Company are extending the constant system of supply in their district, and have completed the arrangements for supplying the houses in one of their districts. The Southwark and Vauxhall Company are constructing covered service reservoirs at Nunhead, to contain 18,000,000 gallons, and are erecting additional engine power for high-pressure constant supply. Additional boilers and works are also being constructed at Hampton. The West Middlesex Company are giving constant supply to a number of houses on the application of the owners, who have provided fittings according to the Board of Trade Regulations. This Company is also constructing extensive works and additional engine-power at Hammersmith and at Hampton to ensure effective constant supply. The Grand Junction Company have completed a high service reservoir near Kilburn, to contain 6,000,000 gallons for constant supply, and have laid a line of main pipes to connect up this reservoir with the works at Campden Hill, and are likewise erecting additional boilers and works at Hampton. The Lambeth Company are also carrying out extensions and improvements to their Works. At Moulsey the construction of reservoirs is being proceeded with, to contain

110,000,000 to 120,000,000 gallons of water, with pumping engines to fill them to a level of 12 feet above the river. When full these reservoirs will contain ten to twelve days' water supply to the district. The water from the reservoirs will run by gravitation through the new conduit to the filters at Ditton, which are in course of extension by the conversion of the reservoirs there into filter beds. The Chelsea Company are proceeding rapidly with the construction of new filter beds at Ditton, and are laying down a new 30-inch pumping main between Kingston and Putney for the constant service supply, besides covering in a reservoir at Putney Heath—not hitherto in use—capable of containing 1,000,000 gallons of filtered water, to improve the supply of the high service.

Street Pavements.—Mr. Haywood, the Engineer and Surveyor to the Commissioners of Sewers of the City of London, in a recent report on the different kinds of pavement now in use, states that, from observations taken, it was found that, of "falls on knees," wood pavement had the greatest proportion, and that asphalt has the fewest of that class of accidents. Of falls on haunches, the asphalt had the largest proportion, and these accidents on it were very largely in excess of those on either of the other pavements, while the wood had the smallest proportion. Of complete falls there were fewest on wood and most on the granite, but the difference between the asphalt and granite was in this respect small. It appeared generally that horses travelling on the wood pavement were on the whole subjected to falls of a character less inconvenient to the general traffic in the street, and also less likely to be injurious to the horses, than those travelling on the other two pavements, and that in this respect the ligno-mineral was superior to the improved wood pavement. It was also noticed that, whatever was the nature of the accident, the horses recovered their feet more easily on wood than they did on either asphalt or granite. On the average of the observations made, the granite was found to be the most slippery, the asphalt the next so, and the wood the least. The conditions of the different pavements varied, however, in some respect with the state of the weather. Further observations than have yet been taken appear necessary before any final and conclusive judgment can be given for or against any one class of pavement.

Steam Economy.—On the 28th of January last Mr. Spence exhibited to a distinguished audience, at Stafford House, a plan by which he proposes to employ the heat of waste steam as a substitute for fuel. This method is founded upon a discovery made by the father of the inventor, that steam liberated at atmospheric pressure, and passed into any saline solution having a boiling temperature higher than that of water, would raise this saline solution to its own boiling-point. In utilising the exhaust steam from a high-pressure engine, Mr. Spence brings it into contact with a solution of caustic soda, which it will raise to a temperature of 375 degrees, or thereabouts, and the heated solution is then circulated through pipes in an ordinary boiler, and its heat is radiated for the purpose of generating steam in the place of heat derived from fresh fuel.

Brighton and Hove Gas-Works.—A paper on this subject was recently read before the Institution of Civil Engineers, by Mr. John Birch Paddon. The site of these works is the widest, most level, and highest part of a tract of shingle lying between the sea and the canal forming the eastern entrance to Shoreham Harbour. This shingle was formerly arrested in its eastward movement by the entrance-works to the harbour, but since the construction of the present westerly entrance it has been greatly wasted by the sea. Between 1865 and 1870, in front of the site of the gas-works, the high-water mark advanced landwards 100 feet. To obtain a deposit of shingle along the sea-front, as a protection, two groynes were first constructed, and others were subsequently built along the coast to be protected. The foundations of the walls of the buildings are laid on concrete, and so extended as to make the proportion of the weight of the superstructure to the bearing surface of 15 cwts. per square foot. The concrete bed under the retort benches is 7 feet 6 inches thick. The retort-house is 286 feet 6 inches long and 80 feet wide.

The chimneys are constructed with the lower parts of brick and the upper parts of wrought-iron, and are sufficiently light to be placed on the benches, so that no floor space is occupied by them: they are 71 feet 6 inches high, 3 feet square at the bottom, and 3 feet in diameter at the top, the least sectional area giving 1 square inch for each lineal foot of retort—a proportion which has proved satisfactory. There are twenty-four benches of retorts, each bench having eight long retorts, there being two mouth-pieces to a retort, or 384 mouth-pieces in all. The retorts are cylinders, 16 inches in diameter and 20 feet long, and each retort will carbonise 1 ton of coal per day. Allowing one-sixth as the number for reserve, the remainder will produce 1,500,000 cubic feet of gas every twenty-four hours, or 300 millions per annum. The engine-house contains four exhausters, each exhauster being driven directly by an independent engine. The entire cost of the works has been about £72,000; and when a proposed second retort-house and coal-store are erected upon the site allotted for them, the total expenditure will amount to £100,000. The works will then be capable of producing 600 millions cubic feet of gas per annum, at a cost of £166 per million on the capital expended.

Concrete Blocks.—The use of concrete blocks, of large size, in the construction of marine works, has, within recent years, come to be greatly extended. An interesting paper, "On the Construction of Harbour and Marine Works with Artificial Blocks of Large Size," by Mr. B. B. Stoney, formed one of the subjects under discussion at the Institution of Civil Engineers last February. The author described a new method of submarine construction with blocks of masonry, or concrete, far exceeding in bulk anything hitherto attempted. The blocks are built in the open air, on a quay or wharf, and, after from two to three months' consolidation, they are lifted by a powerful pair of shear-legs, erected on an iron barge or pontoon. When afloat, they are conveyed to their destination in the foundations of a quay wall, breakwater, or similar structure, where each block occupies several feet of the permanent work, and reaches from the bottom to a little above low-water level. The superstructure is afterwards built on the top of the blocks, in the usual manner. By this method the expenses of coffer-dams, pumping, staging, and similar temporary works are avoided, and economy and rapidity of execution are gained, as well as massiveness of construction, so essential for works exposed to the violence of the sea. There is now being built in this manner an extension, nearly 43 feet in height, of the North Wall Quay, in the port of Dublin. Each of the blocks composing the lower part of the wall is 27 feet high, 21 feet 4 inches wide at the base, 12 feet long in the direction of the wall, and weighs 350 tons. The foundations for the blocks are excavated and levelled by means of a diving-bell. The hull of the floating shears is rectangular in cross section, 48 feet wide, and 130 feet long. The aft end forms a tank, into which water is pumped to balance the weight of the block suspended from the shears at the bow of the vessel.

Dublin Water Supply.—"The Water Supply of the City of Dublin" formed the subject of a paper, by Mr. Parke Neville, at the Institution of Civil Engineers, on the 24th of February. The water supply of the City of Dublin was for several centuries derived from the River Dodder. In 1775 this source was found to be insufficient, and the Corporation entered into contracts with the then Grand Canal Company for a supplemental supply, which were extended in 1808. The water from this source was hard and subject to pollution, and from 1850 to 1857 the question of obtaining a better supply was constantly under consideration. This ultimately led to the appointment of a Royal Commission, and Sir John Hawkshaw was appointed by the Government as Commissioner. The result of his enquiries was the recommendation of the River Vartry as the best source, and works for that object were commenced in November, 1862, and finished in 1868. The Vartry rises at the base of the Sugar-Loaf Mountain, in the county of Wicklow, and the place selected for the storage reservoir is near the village of Roundwood, about 7½ miles below the source of the river. The embankment across the valley, at its deepest point, is 66 feet high, and its total length is 1640 feet. The

greatest depth of water impounded is 60 feet, and the average depth 22 feet. The area of the reservoir is 409 acres, and it is capable of holding 2400 million gallons of water, equal to two hundred days' supply for the city of Dublin and suburbs. Two mains, 48 inches and 33 inches in diameter respectively, are carried through the bank, in a tunnel excavated out of the rock, and arched over. The 48-inch main, which terminates in the bye-wash, is a provision solely for the purpose of being able rapidly to lower the water in the reservoir, if necessary. The 33-inch main conveys the water to a circular receiving-basin, situated at the outer toe of the embankment, and from this basin the water is distributed by side canals on to seven filter-beds. After being filtered, the water is collected into two pure-water tanks, from whence it is carried for about 700 yards in an iron pipe, 42 inches in diameter, with a fall of 6 feet per mile, until it reaches the tunnel, into which it is laid for 120 yards. The tunnel is 4367 yards long, and conveys the water from the natural valley of the Vartry, under a range of hills separating that valley from the districts sloping towards the sea to the east. This tunnel is from 5 feet to 6 feet high and 4 feet wide, with a gradient of 4 feet in a mile. At Callow Hill, the northern or Dublin end of the tunnel, there is a circular receiving-tank, 90 feet in diameter, from which a main, 33 inches in diameter, conveys the water to the distributing reservoir at Stillorgan a distance of 17½ miles. Three tanks relieve the pressure at different points, viz., at Kilmurray, Kilcroney, and Rathmichael. The water is distributed through the streets of the city by mains varying from 27 inches to 18 inches in diameter, which form a zone round the central parts of the city, from which the service-mains diverge. Screw-valves at the intersection of the streets enable the water to be turned off or on, either to repair the mains or to concentrate the pressure in case of fire. Hydrants are placed in every street at intervals of 100 yards, and the system is so perfect that since the introduction of the Vartry water no steam or hand fire-engine has been used to extinguish fires. The total cost of the works has been £610,000, or at the rate of about £1 16s. 6d. per head of the population.

Great Basses Lighthouse.—In 1855 instructions were given for the preparation of a design for the erection of a beacon on the Great Basses Rocks, off the coast of Ceylon. The design submitted was for a cylindrical cast-iron tower, secured within an enlarged basement of masonry, which basement was to be enclosed within an outer casing of cast-iron, and both tower and casing were to be sunk into the rock. After three years' operations, and the expenditure of £40,000, only a few landings had been effected on the rock, and the authorities therefore suspended further proceedings. In June, 1867, the whole question was referred to the Trinity House authorities, who recommended for approval a design prepared by their Engineer, Mr. J. N. Douglas, for a granite structure in which the base of the former structure was proposed to be utilised. The lighthouse, which has now been constructed, consists of a cylindrical base, 30 feet in height and 32 feet in diameter, on which is placed a tower, 67 feet 5 inches in height, 23 feet in diameter at the base, and 17 feet in diameter at the springing of the curve of the cavetto. The thickness of the wall is, at the base of the tower, 5 feet, and at the top 2 feet. The accommodation within consists of six circular rooms, each 13 feet in diameter; and there is also a room, 12 feet in diameter, in the basement, for coals and water, and a rain-water tank below, 7 feet 6 inches in diameter. The tower contains 12,288 cubic feet of granite, and the cylindrical base 25,077 cubic feet, making a total of 37,365 cubic feet, weighing about 2768 tons. Above the tower is a lantern and dioptric revolving apparatus of the first order. The cylindrical 14-feet lantern of the Trinity House has been adopted. The dioptric apparatus has eight panels of refractors, with upper and lower prisms for emitting flashes of red light at intervals of 45 seconds. A 5-cwt. bell, for a signal during foggy weather, is fixed on the lantern gallery. The cost of the building was £64,300, and the light was first exhibited on the 10th of March, 1873, and has since been regularly continued every night from sunset to sunrise.

GEOLOGY.

Physical Geology.—Perhaps the most important contributions to geological science made of late years are the observations of Mr. J. W. Judd, on the "Ancient Volcanoes of the Highlands, and their Relations to the Mesozoic Strata," which he has recently communicated to the Geological Society of London. The vestiges of the secondary strata on the west coast of Scotland have been preserved, like the interesting relics of Pompeii, by being buried under the products of volcanic eruptions. The deposition of these strata was both preceded and followed by exhibitions of volcanic phenomena on the grandest scale. The rocks forming the great *plateaux* of the Hebrides are really the vestiges of innumerable lava-streams, and it is proved that these lavas were of sub-aërial origin, by the absence of all contemporaneous interbedded sedimentary rocks, by the evidently terrestrial origin of the surfaces on which they lie, and by the intercalation among them of old soils, forests, mud-streams, river-gravels, lake-deposits, and masses of unstratified tuffs and ashes. Mr. Judd points out that the great accumulations of igneous products, which in places exhibit a thickness of 2000 feet, must have been ejected from great volcanic mountains, and in the course of his observations he has endeavoured to determine the sites of these old volcanoes, to estimate their dimensions, to investigate their internal structure, and to trace the history of their formation.

Origin of Lake Basins.—Mr. J. Clifton Ward, in discussing the origin of the basins of Derwentwater, Bassenthwaite, Buttermere, Crummock, and Loweswater, pointed out that they were not moraine-dammed lakes, but true rock-basins, and, considering all the features they presented, he was of opinion that the immediate cause of the basins was the onward movement of the old glaciers in the neighbourhood, ploughing up their beds to the comparatively slight depth of the basins which now form the lakes.

Geological Record.—A Record of Geological and Palæontological Literature is being carried out under the direction of the British Association. It is to embrace brief abstracts of all papers published abroad or in the provinces, and will appear regularly in the "Geological Magazine," after which it will be issued in a separate form. Two parts appear in the February and March numbers of the Magazine.

Mr. Whitaker has rendered great service to geologists by preparing lists of papers published on the Geology, Mineralogy, and Palæontology of different counties and districts. He has published those relating to Devonshire, the Hampshire Basin, and Cambridgeshire.

Geology and Parish Boundaries.—The relation of the parish boundaries in the south-east of England to great physical features, and particularly to the chalk escarpment, is a subject to which attention has been called by Mr. Topley. He has shown how the earliest settlements, and the manorial and parochial boundaries, have been dependent upon the original state of the country, whether wooded or open land, and upon the easily accessible water-supply, features which are intimately connected with geological structure.

Palæontology.—Mr. Henry Woodward has described a new star-fish, *Helianthaster filiciformis*, from the Devonian rocks of Harbertonford, South Devon.

Mr. A. Wyatt Edgell has made some additions to the list of fossils from the Budleigh-Salterton pebble-beds. These include species of *Modiolopsis*, *Sanguinolites ?*, *Aviculopecten*, *Pterinca*, *Palæarca*, *Avicula*, *Cleidophorus ?*, *Lunulocardium*, *Ctenodonta*, and *Orthonota ?* The peculiar assemblage of fossils found in the pebbles of this triassic bed was first pointed out by Mr. Vicary, whilst Mr. Salter assigned their parentage to beds on the coast of France. Mr. Edgell notices the accordance between many of the pebbles of Budleigh-Salterton and beds occurring on the opposite side of the Channel, in Brittany. In the discussion which took place after the reading of his paper at the Geological Society of London, Mr. Godwin-Austen observed that, in the same manner as the shingle of Lake Superior is carried away and re-deposited by

shore-ice, so he thought it possible that some action of the same kind might —during a portion of the New Red Sandstone period—have drifted materials off from the French shore of the Triassic lake, and deposited them in this shingle-bed at Budleigh-Salterton.

Dr. H. A. Nicholson has for some time been engaged in collecting and studying the organic remains of the Corniferous limestone and Hamilton formation of the western portion of the province of Ontario, which are considered to be of Devonian age. These deposits are richly fossiliferous: the total number of species comprised in his collections amounts to about 160 or 170, of which between 30 and 40 are apparently new. Of this number no less than 75 are corals, about 40 are Brachiopods, and the remainder are distributed amongst the *Polyzoa*, *Gasteropoda*, *Lamellibranchiata*, *Annelida*, *Trilobita*, and *Crinoidea*.

Diamonds in South Africa.—The occurrence of diamonds in South Africa has during the past few years attracted a considerable amount of public attention. Mr. E. J. Dunn observes that they occur in peculiar circular areas, which he regards as "pipes," which formerly constituted the connection between molten matter below and surface volcanoes. The surrounding country consists of horizontal shales, through which these pipes ascend nearly vertically, bending upwards the edges of the shales at the contact. Mr. Dunn is of opinion that the rock of the pipes was only instrumental in bringing the diamonds to the surface, a large proportion of them being found in a fragmentary state. Prof. Tennant has stated that during the month of November, 1873, no less than £100,000 worth of diamonds were brought from South Africa by three persons. At present about twenty thousand persons are employed in the diamond-fields, and he considers the stones equal to any in the world.

Sub-Wealden Exploration.—In regard to the progress of this boring, Mr. Henry Willett reports (Feb. 12) that the new contractors (the Diamond Rock-Boring Company) are putting forth great energy and skill in the prosecution of their enterprise, so as to make him very sanguine that nothing but want of money will prevent the exploration being extended, if found necessary, to 2000 feet. A depth of 350 feet has now been reached, and the rock there obtained is still Kimeridge clay. We understand that Mr. W. Topley, of the Geological Survey of England, is now resident near the spot where the boring is being made, to examine the specimens as they are brought to the surface.

Proposed Channel Tunnel.—Mr. Prestwich has lately reviewed the geological conditions affecting the construction of a tunnel between France and England, which subject, it will be remembered, was discussed not long ago by Mr. Topley in the "Quarterly Journal of Science." Mr. Prestwich pointed out the geological features of all the strata between Harwich on the one side, and between Ostend and St. Valery on the other side of the Channel, and stated his opinion that the most feasible plan, and one that would be perfectly practicable, so far as safety from an influx of sea-water was concerned, was to drive a tunnel through the Palæozoic rocks under the Channel between Blanc Nez and Dover.

Geological Society of London.—At the anniversary meeting of the Geological Society, held on February 20th, the Duke of Argyll announced the award of the Wollaston Gold Medal to Professor Heer, of Zurich, in acknowledgment of his extensive researches in fossil botany and entomology. The balance of the proceeds of the Wollaston Donation Fund was given to Dr. H. Nyst, of Brussels, in recognition of his admirable researches upon the Molluscan and other fossil remains of his native country. The Murchison Medal was awarded to Dr. Bigsley in recognition of his long and valuable labours in that department of geology and palæontology with which the name of Murchison is more particularly connected. The balance of the proceeds of the Murchison Geological Fund was divided between Mr. Ralph Tate and Mr. Alfred Bell as a testimony of the value set by the Society upon their palæontological researches. John Evans, Esq., F.R.S., &c., was elected as President for the ensuing year.

PHYSICS.

Microscopy.—The Quekett Microscopical Club has recently been presented with a series of insect preparations in balsam, from Ceylon. They are remarkable, inasmuch as the usual eviscerating and laying-out processes have not been adopted, but the insects mounted as much as possible in their natural position, and with the minimum amount of compression: the preparations are in many cases very thick, but this is no disadvantage, but rather the contrary, for observations with low powers and the binocular microscope. The insects were merely dried between the leaves of a book, and then mounted in balsam, sometimes after a soaking in turpentine; *liquor potassæ* has in no instance been employed. The hardening of the balsam has been effected entirely by exposure to the sun; the collection is totally free from milkiness from damp, and the penetration of the balsam is perfect. This mode of preparation is, of course, only available in tropical climates. Such preparations would probably be best made here by drying the insects by immersion in absolute alcohol, then soaking in oil of cloves, and, when the preparation has cleared, mounting in balsam. This process is much used on the Continent for anatomical subjects, frequently stained and injected, and is but little known in this country: it will be found invaluable where other modes of drying cannot be made available.

In illustrating a paper on the "Life-History of Monads," read before the Royal Microscopical Society, the Rev. W. H. Dallinger, F.R.M.S., executed the drawings in a manner which rendered them available for lecture illustration with the magic-lantern. The material chosen is finely-ground glass, upon which the drawing is made as readily as upon paper: the camera-lucida, or other instrument, is available for obtaining the sketch from the microscope. The pencils used should be harder than those employed in drawing upon paper; the engraver's 6 H will prove useful, HB being strong enough for the deepest shadows. When the drawing is finished, it is to be rendered transparent by the application of Canada balsam, thinned with benzol to the consistence of cream: this is poured on the plate, and evenly distributed, somewhat in the manner of collodion in photography. When hard, the varnished surface is protected from injury by a glass fastened on it by strips of paper at the edges, with small pieces of card at the corners to prevent contact. Water-colour is available upon the ground-glass surface. The process will be found in every way easier than the usual mode of producing magic-lantern slides, and equally effective.

The "Sand-blast" process has been successfully employed by Mr. H. F. Hailes, of the Quekett Club, for excavating hollows in glass slides to be used as cells. For dry mounting they answer admirably, and the roughness of the bottom is no hindrance to mounting objects in balsam, as the lower surface of the cell is rendered perfectly transparent by contact with the mounting material.

Messrs. Underhill and Allen communicate to the March number of "Science Gossip" the following formula for glycerin jelly:—"Soak ½ oz. best amber gelatin (as sold by the chemist or grocer) in 1 oz. distilled water; when it has absorbed all the water, put it in a Florence flask with 50 grains of powdered chloride of barium; warm it in a water-bath, and agitate till the barium salt is dissolved. Allow it to cool below 35° C., and while still fluid add 1 oz. of Price's glycerin and a teaspoonful of white of egg; shake until well mixed, replace in the water-bath, and boil at full speed until the albumen *completely* separates from the jelly in the form of a single lump. Now filter through well-washed fine flannel, and it should be as clear as crystal; but if through mismanagement it be a little cloudy, filter it again, this second time using filtering-paper, and placing the funnel with the jelly, &c., in a '*cool*' oven. During the coagulation of the albumen, the jelly must on no account be stirred. It is well to beat up the white of egg before use, lest it should be stringy. The jelly made after this recipe is of the proper consistency for entomological objects, but for delicate vegetable structures it should be softened by adding to it a third of its volume of a mixture of equal parts of glycerin

and distilled water. Put the jelly into test-tubes ½ inch in diameter, and when you wish to mount a slide warm the upper part of the tube: in this way you can pour out any quantity free from bubbles. It is perhaps well to put a trace of varnish, or some essential oil, on the corks, lest they should get mouldy. The chloride of barium prevents fungi in the jelly, and is the best preservative we know of; but something is absolutely necessary. By all means avoid putting alcohol, creosote, &c., in the jelly, as they dissolve varnishes, and also spoil the colour of some objects." The hints on mounting in this paper are of great value.

An object-glass of American manufacture has arrived in London, engraved "R. B. Tolles, Boston. Immersion, $\frac{1}{6}$th. 180°!! Balsam angle, 98." It performs well, defining splendidly, but is wanting—as are some good English objectives—in flatness of field. Why stop at 180°? Surely an enlightened citizen of a free and independent country will not be trammelled by the laws of mathematics and optics.

Heat.—In a paper communicated to the Royal Society, "On the Action of Heat on Gravitating Masses," the Editor of this Journal has recorded experiments which arose from observations made when using the vacuum-balance, described by the author in his paper "On the Atomic weight of Thallium,"* for weighing substances which were of a higher temperature than the surrounding air and the weights. There appeared to be a diminution of the force of gravitation, and experiments were instituted to render the action more sensible, and to eliminate sources of error. After an historical *résumé* of the state of our knowledge on the subject of attraction or repulsion by heat, the author describes numerous forms of apparatus successively more and more delicate, which enabled him to detect, and then to render very sensible, an action exerted by heat on gravitating bodies, which is not due to air-currents, or to any other known force. The following experiment with a balance made of a straw beam with pith-ball masses at the ends enclosed in a glass tube, and connected with a Sprengel pump, may be quoted from the paper:—"The whole being fitted up as here shown, and the apparatus being full of air to begin with, I passed a spirit-flame across the lower part of the tube at *b*, observing the movement by a low-power micrometer; the pith-ball (*a*, *b*) descended slightly, and then immediately rose to considerably above its original position. It seemed as if the true action of the heat was one of attraction, instantly overcome by ascending currents of air. . . . 31. In order to apply the heat in a more regular manner, a thermometer was inserted in a glass tube, having at its extremity a glass bulb, about 1½ inches diameter; it was filled with water, and then sealed up. . . . The water was kept heated to 70° C., the temperature of the laboratory being about 15° C. 32. The barometer being at 767 millims., and the gauge at zero, the hot bulb was placed beneath the pith-ball at *b*. The ball rose rapidly; as soon as equilibrium was restored, I placed the hot-water bulb above the pith-ball at *a*, when it rose again, more slowly, however, than when the heat was applied beneath it. 33. The pump was set to work, and when the gauge was 147 millims. below the barometer, the experiment was tried again; the same result, only more feeble, was obtained. The exhaustion was continued, stopping the pump from time to time, to observe the effect of heat, when it was seen that the effect of the hot body regularly diminished as the rarefaction increased, until when the gauge was about 12 millims. below the barometer the action of the hot body was scarcely noticeable. At 10 millims. below it was still less; whilst when there was only a difference of 7 millims. between the barometer and the gauge, neither the hot-water bulb, the hot rod, nor the spirit-flame caused the ball to move in an appreciable degree. The inference was almost irresistible that the rising of the pith was only due to currents of air, and that at this near approach to a vacuum the residual air was too highly rarefied to have power in its rising to overcome the inertia of the straw beam and the pith balls. A more delicate instrument would doubtless show traces of movement

* Phil. Trans., cxliii., part 1, p. 277.

at a still nearer approach to a vacuum; but it seemed evident that when the last trace of air had been removed from the tube surrounding the balance (when the balance was suspended in empty space only), the pith-ball would remain motionless wherever the hot body were applied to it. 34. I continued exhausting. On next applying heat, the result showed that I was far from having discovered the law governing these phenomena; the pith-ball rose steadily, and without that hesitation which had been observed at lower rarefactions. With the gauge 3 millims. below the barometer, the ascension of the pith when a hot body was placed beneath it was equal to what it had been in air of ordinary density; whilst with the gauge and barometer level its upward movements were not only sharper than they had been in air, but they took place under the influence of far less heat; the finger, for example, instantly sending the ball up to its fullest extent." A piece of ice produced exactly the opposite effect to a hot body. Numerous experiments are next given to prove that the action is not due to electricity. The presence of air having so marked an influence on the action of heat, an apparatus was fitted up in which the source of heat (a platinum spiral rendered incandescent by electricity) was inside the balance-tube instead of outside it as before; and the pith-balls of the former apparatus were replaced by brass balls. By careful management, and turning the tube round, the author could place the equipoised brass pole either over, under, or at the side of the source of heat. With this apparatus it was intended to ascertain more about the behaviour of the balance during the progress of the exhaustion, both below and above the point of no action, and also to ascertain the pressure corresponding with this critical point. After describing many experiments with the ball in various positions in respect to the incandescent spiral, and at different pressures, the general result appeared to be expressed by the statement that the tendency in each case was to bring the centre of gravity of the brass ball as near as possible to the source of heat, when air of ordinary density, or even highly rarefied, surrounded the balance. The author continues:—"44. The pump was then worked until the gauge had risen to 5 millims. of the barometric height. On arranging the ball above the spiral (and making contact with the battery), the attraction was still strong, drawing the ball downwards a distance of 2 millims. The pump continuing to work, the gauge rose until it was within 1 millim. of the barometer. The attraction of the hot spiral for the ball was still evident, drawing it down when placed below it, and up when placed above it. The movement was, however, much less decided than before; and in spite of previous experience (33, 34) the inference was very strong that the attraction would gradually diminish until the vacuum was absolute, and that then, and not till then, the neutral point would be reached. Within 1 millimetre of a vacuum there appeared to be no room for a change of sign. 45. The gauge rose until there was only half a millimetre between it and the barometer. The metallic hammering heard when the rarefaction is close upon a vacuum commenced, and the falling mercury only occasionally took down a bubble of air. On turning on the battery current, there was the faintest possible movement of the brass ball (towards the spiral) in the direction of *attraction*. 46. The working of the pump was continued. On next making contact with the battery no movement could be detected. The red-hot spiral neither attracted nor repelled; I had arrived at the critical point. On looking at the gauge I saw it was level with the barometer. 47. The pump was now kept at full work for an hour. The gauge did not rise perceptibly, but the metallic hammer increased in sharpness, and I could see that a bubble or two of air had been carried down. On igniting the spiral, I saw that the critical point had been passed. The sign had changed, and the action was faint but unmistakable *repulsion*. The pump was still kept going, and an observation was taken from time to time during several hours. The repulsion continued to increase. The tubes of the pump were now washed out with oil of vitriol,* and the working was continued for an hour. 48. The action of the incandescent spiral was now found to be energetically *repellent*,

* This can be effected without interfering with the exhaustion.

whether it was placed above or below the brass ball. The fingers exerted a repellent action, as did also a warm glass rod, a spirit-flame, and a piece of hot copper." In order to decide once for all whether these actions really were due to air-currents, a form of apparatus was fitted up, which, whilst it would settle the question indisputably, would at the same time be likely to afford information of much interest. By chemical means a vacuum was obtained in an apparatus so nearly perfect that it would not carry a current from a Ruhmffork's coil when connected with platinum wires sealed into the tube. In such a vacuum the repulsion by heat is decided and energetic. An experiment is next described, in which the rays of the sun, and then the different portions of the solar spectrum, are projected into the delicately suspended pith-ball balance. *In vacuo* the repulsion is so strong as to cause danger to the apparatus, and resembles that which would be produced by the physical impact of a material body. Experiments are next described in which various substances are used as the gravitating masses. Amongst these are ivory, brass, pith, platinum, gilt pith, silver, bismuth, selenium, copper, mica (horizontal and vertical), charcoal, &c. The behaviour of a glass beam with glass ends in a chemical vacuum, and at lower exhaustion, is next accurately examined, when heat is applied in different ways. On suspending the light index by means of a cocoon fibre in a long glass tube, furnished with a bulb at the end, and exhausting in various ways, the author finds that the attraction to a hot body in air, and the repulsion from a hot body *in vacuo*, are very apparent. Speaking of Cavendish's celebrated experiment, the author says that he has experimented for some months on an apparatus of this kind, and gives the following outline of one of the results he has obtained :—" A heavy metallic mass, when brought near a delicately suspended light ball, attracts or repels it under the following circumstances.

" I. *When the ball is in air of ordinary density.*

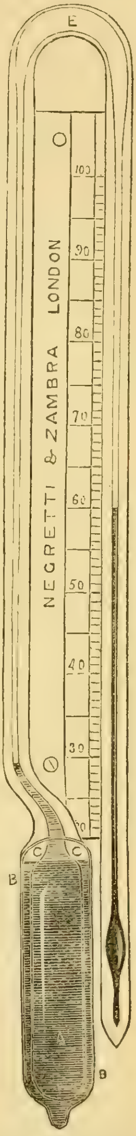
a. If the mass is *colder* than the ball, it *repels* the ball.
b. If the mass is *hotter* than the ball, it attracts the ball.

" II. *When the ball is in a vacuum.*

a. If the mass is *colder* than the ball, it *attracts* the ball.
b. If the mass is *hotter* than the ball, it *repels* the ball."

The author continues :—" The density of the medium surrounding the ball, the material of which the ball is made, and a very slight difference between the temperatures of the mass and the ball, exert so strong an influence over the attractive and repulsive force, and it has been so difficult for me to eliminate all interfering actions of temperature, electricity, &c., that I have not yet been able to get distinct evidence of an independent force (not being of the nature of heat) urging the ball and the mass together. " Experiment has, however, showed me that, whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, it would seem that such an action as was obtained by Cavendish, Reich, and Bailey, should be rendered evident." After discussing the explanations which may be given of these actions, and showing that they cannot be due to air-currents, the author refers to evidences of this repulsive action of heat, and attractive action of cold, in Nature. In that portion of the sun's radiation which is called heat, we have the radial repulsive force possessing successive propagation required to explain the phenomena of comets and the shape and changes of the nebulæ. To compare small things with great (to argue from pieces of straw up to heavenly bodies), it is not improbable that the attraction now shown to exist between a cold and a warm body will equally prevail when, for the temperature of melting ice is substituted the cold of space, for a pith ball a celestial sphere, and for an artificial vacuum a stellar void. In the radiant molecular energy of cosmical masses may at last be found that " agent acting constantly according to certain laws," which Newton held to be the cause of gravity.

Messrs. Negretti and Zambra have recently communicated to the Royal Society the description of a new Deep-sea Thermometer. For the purpose of ascertaining the temperature of the sea at various depths, and on the bottom itself, a peculiar thermometer was, and is, used, having its bulb protected by an outer bulb or casing, in order that its indications may not be vitiated by the pressure of the water at various depths, that pressure being about 1 ton per square inch to every 800 fathoms. This thermometer, as regards the protection of the bulb and its non-liability to be affected by pressure, is all that can be desired; but unfortunately the only thermometer available for the purpose of registering temperature and bringing those indications to the surface is that which is commonly known as the Six's thermometer—an instrument acting by means of alcohol and mercury, and having movable indices with delicate springs of human hair tied to them. This form of instrument registers both maximum and minimum temperatures, and as an ordinary out-door thermometer it is very useful; but it is unsatisfactory for scientific purposes, and for the object which it is now used it leaves much to be desired. Thus the alcohol and mercury are liable to get mixed in travelling, or even by merely holding the instrument in a horizontal position; the indices also are liable either to slip if too free, or to stick if too tight. A sudden jerk or concussion will also cause the instrument to give erroneous readings, by lowering the indices if the blow be downwards, or by raising them if the blow be upwards. Besides these drawbacks, the Six's thermometer causes the observer additional anxiety on the score of inaccuracy; for, although we get a *minimum* temperature, we are by no means sure of the point where this minimum lies. Messrs. Negretti and Zambra have constructed an instrument on a plan different from that of any other self-registering thermometers. Its construction is most novel, and may be said to overthrow our previous ideas of handling delicate instruments, inasmuch as its indications are only given by upsetting the instrument. Having said this much, it will not be very difficult to guess the action of the thermometer; for it is by upsetting or throwing out the mercury from the indicating column into a reservoir, at a particular moment and in a particular spot, that we obtain a correct reading of the temperature at that moment and in that spot. The instrument has a protected bulb thermometer, like a syphon with parallel legs, all in one piece, and having a continuous communication, as in the annexed figure. The scale of this thermometer is pivotted on a centre, and being attached in a perpendicular position to a simple apparatus (presently described), is lowered to any depth that may be desired. In its descent the thermometer acts as an ordinary instrument, the mercury rising or falling according to the temperature of the stratum through which it passes; but so soon as the descent ceases, and a reverse motion is given to the line, so as to pull the thermometer to the surface, the instrument turns once on its centre, first bulb uppermost, and afterwards bulb downwards. This causes the mercury, which was in the left-hand column, first to pass into the dilated syphon bend at the top, and thence into the right-hand tube, where it remains, indicating on a graduated scale the exact temperature at the time it was turned over. The woodcut shows the position of the mercury after the instrument has been thus turned on its centre. A is the bulb; B the outer coating or protecting cylinder; C is the space of rarefied air, which is reduced if the outer casing be compressed; D is a small glass plug, on the principle of



Negretti and Zambra's patent maximum thermometer, which cuts off, in the moment of turning, the mercury in the column from that of the bulb in the tube, thereby ensuring that none but the mercury in the tube can be transferred into the indicating column; E is an enlargement made in the bend, so as to enable the mercury to pass quickly from one tube to another in revolving; and F is the indicating tube, or thermometer proper. In its action, as soon as the thermometer is put in motion, and immediately the tube has acquired a slightly oblique position, the mercury breaks off at the point D, runs into the curved and enlarged portion E, and eventually falls into the tube F, when this tube resumes its original perpendicular position. The contrivance for turning the thermometer over may be described as a short length of wood or metal having attached to it a small rudder or fan: this fan is placed on a pivot in connection with a second; on the centre of this is fixed the thermometer. The fan or rudder points upwards in its descent through the water, and necessarily reverses its position in ascending. This simple motion, or half-turn of the rudder, gives a whole turn to the thermometer, and has been found very effective. Various other methods may be used for turning the thermometer, such as a simple pulley with a weight which might be released on touching the bottom, or a small vertical propeller which would revolve in passing through the water. Messrs. Negretti and Zambra have also adopted a very simple and inexpensive clock-work to their thermometer, and by these means an observer may have a record of the exact temperature at any hour of the day or night. We need hardly say of what utility the instrument will prove to meteorologists, and even manufacturers, to whom an exact *record* of temperature is of importance. Hitherto we have had no simple and inexpensive instrument adapted for this purpose: the thermograph in use at most observatories is an elaborate and expensive apparatus, which, in connection with photography, will record on paper the temperature during day or night; it necessitates the use of gas, or any artificial light, and of course is only available to persons who can have a building specially adapted for it.

Electricity.—At a recent meeting of the Manchester Literary and Philosophical Society Professor Osborne Reynolds, M.A., read a paper "On the Bursting of Trees and Objects struck by Lightning." In a previous paper on this subject he stated that the tube which was burst by a discharge from a jar would probably withstand an internal pressure from 2 to 5 tons on the square inch; and he made use of the expression the tube might be fired like a gun without bursting. These statements were based on the calculated strength of the tube, and with a view to show that there was no mistake, the author tried it in the following manner:—He made three guns of the same tube. No. 1, which was 6 inches long, had its end stopped with a brass plug containing the fuze hole. Nos. 2 and 3 were 6 inches long, and had their breeches drawn down so as only to leave a fuze hole. These tubes were loaded with gunpowder and shotted with slugs of wire which fitted them, and which were all ¾ inch long. No. 1 was first fired with ½ inch of powder, the shot penetrated ¼ inch into a deal board, and the gun was uninjured. No 2. was then fired with 1½ inches of powder, and the shot went through the 1 inch deal board and ½ inch into some mahogany behind, thus penetrating altogether 1½ inches; the tube, however, was burst to fragments. Some of these were recovered, and although they were small they did not show cracks and signs of crushing like those from the electrical fracture. No. 3 was then fired with ¾ inch of powder, and the shot penetrated ½ inch into the deal board. It was again fired with 1 inch of powder, and the shot penetrated 1 inch into the deal. Again it was a third time fired with 1¼ inches of powder, when it burst, and the shot only just dented the wood. These experiments seem to prove conclusively the great strength of the tube and the enormous bursting force of the electrical discharge.

At the first meeting of the Physical Society of London the Chairman (Dr. Gladstone, F.R.S.) gave a brief description of the objects and organisation of the Society, and announced that ninety-nine gentlemen had already expressed their desire to join the Society as original members.

Mr. J. A. Fleming, B.Sc., read a paper on the "Contact Theory of the Battery." After discussing the most recent views regarding the contact and chemical theories, Mr. Fleming exhibited the action of his new battery, in which metallic contact of dissimilar metals is completely avoided. The battery consisted of thirty test-tubes of dilute nitric acid alternating with the same number of tubes of pentasulphide of sodium, all well insulated. Bent strips of alternate lead and copper connected the neighbouring tubes. By this device the terminal poles are of the same metal. On connecting with a coarse galvanometer, the needle was violently and permanently deflected. Tested by the quadrant electrometer, the potential was shown to increase regularly with the number of cells. The sixty cells on first immersion showed a potential exceeding that of 14 Daniell's cells. The principle upon which the action depends is that, in the acid, lead is positive to copper; in the sulphide it is negative. Mr. Fleming further showed how, by using the single liquid, nitric acid, and the single metal, iron, a single battery could be constructed, provided one-half of each iron strip were rendered passive. In this form, also, no metallic contact occurred.

Prof. F. Guthrie exhibited experiments illustrating the distribution of a galvanic current on entering and leaving a conducting medium. This was shown in the case of solids by the stratification of iron-filings on sheets of copper and lead. The effect of the distribution on a magnetic needle which is hung near a conducting vertical sheet in the magnetic meridian—into the upper horizontal edge of which a current enters, and out of which it passes at the same elevation—is to alter the direction of the needle's direction of turning, according as the needle is lowered or raised. At a distance from the upper edge of one-third the distance of the interval between the poles, the needle is at rest. A similar effect was shown in a liquid conductor.

The following are the officers of the Society for the first Session:—President, J. H. Gladstone, Ph.D., F.R.S. Vice-Presidents, Prof W. G. Adams, F.R.S.; Prof. G. C. Foster, F.R.S. Secretaries, Prof. E. Atkinson, Ph.D., York Town, Surrey; Prof. A. W. Reinold, M.A., Royal Naval College, Greenwich. Treasurer, Prof. E. Atkinson, Ph.D. Demonstrator, Prof. Frederick Guthrie. Other Members of the Council, W. Crookes, F.R.S.; Prof. A. Dupré; Prof. T. M. Goodeve, M.A.; Prof. O. Henrici; B. Loewy; E. J. Mills, D.Sc.; H. Sprengel, Ph.D.

Dr. Geissler, of Bonn, Germany, whose name is inseparably associated with some of the most beautiful experiments that can be performed by the agency of electricity, makes an electrical vacuum tube that may be lighted without either induction coil or frictional machine. It consists of a tube an inch or so in diameter, filled with air as dry as can be obtained, and hermetically sealed after the introduction of a smaller exhausted tube. If this outward tube be rubbed with a piece of flannel, or any of the furs generally used in exciting the electrophorus, the inner tube will be illumined with flashes of mellow light. The light is faint at first, but gradually becomes brighter and softer. It is momentary in duration; but if the tube be rapidly frictioned, an optical delusion will render it continuous. If the operator have at his disposal a piece of vulcanite, previously excited, he may, after educing signs of electrical excitement within the tube, entirely dispense with the use of his flannel or fur. This will be found to minister very much to his personal ease and comfort. He may continue the experiments, and with enhanced effect, by moving the sheet of vulcanite rapidly up and down at a slight distance from the tube. This beautiful phenomenon is an effect of induction.

In a note on a remarkable production of light in grinding of hard stones, Dr. Nöggerath refers to a visit made to some agate works at Oberstein and Idar, in which various kinds of hard stone are pressed by the workmen (with their hands) against quickly-revolving grindstones. The transparent stones become pervaded throughout with a yellowish-red light, like that of red-hot iron. Opaque stones give a red light at the place of contact, with halo and sparks. Dr. Nöggerath thinks the phenomena worth

studying by physicists, especially as regards development of heat and electricity.

In some researches on change in pitch of tones through movement of the source of sound, and determination, by this means of the velocity of sound, Dr. Schüngel experimented with two tuning forks, No. 1 giving 512, and No. 2 508 vibrations in a second, Sounded together they gave four beats in a second. But suppose No. 2 moved towards the observer (situated beside No. 1), its quantity of vibrations would be increased and the number of beats diminished. Dr. Schüngel sought to measure—(1) the time in which a certain number of successive beats was audible; and (2) the velocity of the moved fork. His apparatus (which was electrical) may be briefly described:—A seconds pendulum at each swing closed a circuit, which, through a relay, caused a series of dots to be marked on a telegraph strip at intervals corresponding to seconds. By pressing a key another battery circuit could be closed, which had two effects: part of the current went to the relay, and produced a line in the telegraph paper so long as the key was held down; but the greater part went through an electro-magnet, which attracted an armature at one end of a lever, having at its other end a roller rotated by a cord from a fly-wheel. The roller was thus pressed against the edge of a disc, which, thus set in motion, wound in, by a cord about its axis, a little wagon bearing the tuning fork (No. 2) with its case towards the observer. The method, with some suggested modifications, is commended to the attention of physicists for an accurate determination of the velocity of sound.

TECHNOLOGY.

Count Sokolnicki, a proprietor of vineyards at Medoc, states that a chemist, so-called, is selling to the wine-forgers of the Gironde a liquid of which a few drops suffice to colour a wine. An œnanthic liquor, simulating the bouquet of Medocs, is sold openly at Bordeaux. A solution of sugar is allowed to ferment on the pressed grapes, the colour and the flavour are added, and with these materials wines of the best growths are counterfeited.

For the manufacture of permanent beer M. Pasteur recommends the use of a pure yeast,—the mode of preparing which he does not describe,—free from vibriones, bacteria, *Mycoderma aceti*, &c. With such yeast, the process of fermentation can be carried on in the absence of air, or in the presence only of limited quantities of pure air. Beers thus made can, he declares, be preserved for an indefinite length of time, even at temperatures of 20° to 25° C.

M. Paul has effected an improvement in photo-lithography. He produces a positive image on paper covered with a layer of albumen mixed with a concentrated solution of bichromate of potash. After a sufficient insolation under the negative, the paper is covered with lithographic ink, and then immersed in cold water to dissolve the unaltered albumen.

As a test for the colouring matter of wines, M. de Cherville gives the following process:—Pour into a glass a small quantity of the wine under examination, and dissolve in it a morsel of potassa. If there is no deposit, and if the wine takes a greenish tint, it has not been artificially coloured. If a violet deposit has been formed, the wine has been coloured with elderberries or mulberries; if the deposit is red, beet-root or peach-wood has been used; and if violet-red, logwood. If the sediment is violet-blue, privet berries have been employed; and if a bright violet, litmus.

A paper "On Coloured Tapers," by Mr. James MacFarlane, Assistant to the Professor of Chemistry, St. Andrews, was recently read before the Chemical Section of the Glasgow Philosophical Society. The author detailed a series of experiments which he had prosecuted for the purpose of determining the nature of the colouring matter in the green and red wax tapers. He distinctly ascertained that the former owed their colour to the presence of Scheele's green (arsenite of copper). Their average weight was 2 grms., and the average time occupied in burning was seventeen minutes. Guided by the

colour and by the alliaceous odour evolved during combustion, he had no difficulty in pronouncing that arsenic was present; its presence was experimentally determined, and its quantity estimated to be 0·60 per cent of the taper, equal to 0·35 grm., or 5·43 grs. of arsenious acid—quite enough to poison two people if taken directly in the solid form. The red tapers weighed, on an average, about 8·94 grms., and burned seventeen minutes, leaving 3 milligrms. of ash totally devoid of metallic appearance. Mercury, existing as vermillion, was found by Reinsch's process, and its quantity was afterwards carefully determined. The amount of mercuric sulphide ultimately collected, washed, and dried, was 1·66 per cent. In one series of experiments the following results were arrived at—white, yellow, blue, red, and green tapers, being experimented upon:—

White.—Perfectly harmless; little ash.

Yellow.—Harmless; coloured with chromate of lead; ash, metallic.

Blue.—Harmless; coloured with ultramarine.

Red.—Highly poisonous, containing 1·93 per cent of vermillion; the tapers very highly coloured; slight ash.

Green.—Poisonous; colour due to arsenic; metallic ash; quantity of arsenic not determined, but probably about 1 per cent.

These tapers burned, on an average, twelve minutes, and in number and quality were much superior to the first, which were of the spiral character. The table is a summary of the results of the examination of the spiral tapers. The author afterwards proceeded to consider the effect arising, or which might arise, from the use of coloured wax tapers, and the inhalation of the vapours resulting from their combustion.

	Red.	Green.
Time occupied in burning	12 mins.	17 mins.
Weight	0·93 grms.	2 grms.
Percentage of wax	72·90	71·30
Percentage of wick	25·44	26·89
Weight of wax, per taper	24·85	22·53
Weight of wick, per taper..	8·67	8·49
Percentage of arsenious acid	—	1·81
Percentage of vermillion	1·66 to 1·93	—

The same author submitted a communication on arsenical papers, in the course of which he reviewed the theories and cases for and against the alleged unhealthiness of rooms papered with hangings having Scheele's green as one of their colouring matters. He mentioned several cases of severe illness, and even of death, distinctly traceable to the inhalation of the green arsenical compound used in the preparation of the cheaper kinds of paper-hangings.

Referring to the opinion expressed by Mr. S. Barber on page 36 of the "Quarterly Journal of Science," for January, 1874, that mock suns outside the sun are an unusual phenomenon, Mr. T. W. Backhouse writes to say this is not the case, and that Flammarion's book, "The Atmosphere," states that mock suns are only on the halo when the sun is low, and that as its altitude increases they gradually emerge from it. With reference to the halo of 90°, which Mr. Barber says he believes is not seen in summer, our correspondent says that, if the halo about 90° *in diameter* is alluded to, he has seen it in summer, viz., in 1869, on June 11th and on August 10th. Both these days were, however, cool, with a maximum temperature of 58°.

LIST OF PUBLICATIONS AND PERIODICALS RECEIVED FOR REVIEW.

Cholera: how to Avoid and Treat it. By Henry Blane, M.D., M.R.C.S. *Henry S. King and Co.*

Catalogue of Stars observed at the United States Naval Observatory during the Years 1845 to 1871. Prepared by Prof. M. Varnall, U.S.N. *Washington:* 1873.

Treatise on Practical, Solid, or Descriptive Geometry. By W. T. Pierce. *Longmans and Co.*

The Naturalist in Nicaragua. By Thomas Belt, F.G.S. *John Murray.*

Personal Recollections, from Early Life to Old Age, of Mary Somerville. By her Daughter, Martha Somerville. *John Murray.*

The Galvanometer and its Uses: a Manual for Electricians and Students. By C. H. Haskins. *New York: D. Van Nostrand.*

Reliquæ Aquitanicæ; being Contributions to the Archæology and Palæontology of Périgord and the Adjoining Provinces of Southern France. By Edouard Lartet and Henry Christy. Edited by Thomas Rupert Jones, F.R.S., &c. *Williams and Norgate.*

Hydraulics of Great Rivers: the Paraná, the Uruguay, and the La Plata Estuary. By J. J. Révy, Memb. Inst. C.E., Vienna and London. *E. and F. N. Spon.*

Substance of the Work entitled "Fruits and Farinacea the Proper Food of Man. Edited by Emeritus Prof. F. W. Newman. *F. Pitman.*

Our Ironclads and Merchant-Ships. By Rear-Admiral E. Gardiner Fishbourne, C.B. *E. and F. N. Spon.*

The Nature and Formation of Flint and Allied Bodies. By M. Hawkins Johnson, F.G.S.

Legal Responsibility in Old Age. By George M. Beard, A.M., M.D. *New York: T. L. Clacher.*

An Elementary Treatise on Steam. By John Perry, B.E. *Macmillan and Co.*

The Psychology of Scepticism and Phenomenalism. By James Andrews. *Glasgow: J. Maclehose.*

Principles of Mental Physiology, with their Applications to the Training and Discipline of the Mind and the Study of its Morbid Conditions. By W. B. Carpenter, M.D., LL.D., F.R.S., &c. *Henry S. King and Co.*

Handbook of Natural Philosophy. By Dionysius Lardner, D.C.L. Edited by Benjamin Loewy, F.R.G.S. *Lockwood and Co.*

Inklings of Aërial Autonometry. By William Houlston. *Simpkin, Marshall, and Co.*

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PROGRESS IN SCIENCE,

(Including the Proceedings of Learned Societies at Home and Abroad, and Notices of Recent Scientific Literature).

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THE QUARTERLY
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JULY, 1874.

I. THE POLE STAR AND THE POINTERS.

By Lieut.-Colonel A. W. Drayson, R.A., F.R.A.S.

NEARLY every educated person is sufficiently acquainted with the familiar objects in the heavens to be able to recognise the constellation known as the "great bear." Two stars of this constellation are termed "the pointers," because they point towards a third star called the "pole star." The great bear, the pointers, and the pole star may therefore be called well-known objects; for as these stars always remain above the horizon in our latitude, they may be seen every clear night throughout the year.

Among the hundreds of thousands of persons who look at the pole star and the pointers, probably a hundred times a year, we have hitherto found only about four who have remarked a singular and interesting fact connected with these objects. When, however, we have pointed out the interesting fact, then many persons have asserted that they had noticed the peculiarity we referred to, but that want of confidence in their own observational powers had caused them to conclude that what they fancied they saw was an optical delusion, and not an actual fact. Results, however, of a valuable kind emanate from the peculiarity we shall here refer to; and though in the present article we are compelled to treat the subject in a somewhat popular or superficial manner, it is yet connected with problems of considerable difficulty and of great practical utility, which have hitherto to a considerable extent escaped notice, in consequence of the science of Geometry having in modern times been greatly neglected.

Many years ago it was our fate to make a voyage from England towards the Southern Hemisphere; it was part of our duty to keep what are called "watches" on board ship. We had sometimes to remain on deck from 8 o'clock P.M. till midnight, and at other times from 4 A.M. till 8 A.M., and during these hours we had every opportunity of observing

the changes in various celestial bodies, due to the half rotation of the earth, which had occurred from 8 P.M. to 8 A.M.

The pole star and the pointers particularly attracted our attention, for we could easily perceive how the pole star appeared to sink nearer and nearer to the horizon as we passed each day two or three degrees nearer the equator, and we realised one of the first elementary laws of Astronomy, viz., that the altitude of the pole above the northern horizon was always equal to the latitude of the place from which the pole was seen. In 52° N. latitude the altitude of the pole was therefore 52°, whilst in N. latitude 40° the altitude of the pole is only 40°, and so on.

Whilst observing the pole star and the pointers, at intervals of about twelve hours, and when consequently the pointers were at one time east of the pole star and at other times west of it, we noticed that at one time, and under one condition, the pointers appeared to point more directly towards the pole star than they did at other times. When this peculiarity was at first noticed we believed it was in consequence of our want of observation; for we naturally argued that, as the fixed stars did not alter their relative position as regards each other, it must follow that if the pointers pointed towards the pole star at one time, they must do so with equal exactitude at another time. To imagine that the mere rotation of the earth on its axis could cause any alteration in the relative position of three stars in the heavens seemed almost an absurdity, and although night after night, and morning after morning, we observed that it really appeared as if the pointers did point in a variable manner as regards the pole star, yet the fact was passed over—as facts too often are—by those who cannot account for them.

It was several years after our first notice of the pole star and the pointers that our attention was again directed to these celestial bodies, and we once more noticed the same facts as those already referred to, and very shortly recognised the law or cause which produced the appearance, and found how more than one interesting problem depended thereon.

In order that the whole of the problems resulting from the above law should be thoroughly understood, the reader ought to be acquainted with what we may term the geometry of the sphere, and also the various terms used in Astronomy, but to *comprehend* these problems will require but average intelligence, a careful perusal of the following pages, and a sufficient amount of imagination to picture, as it were, on

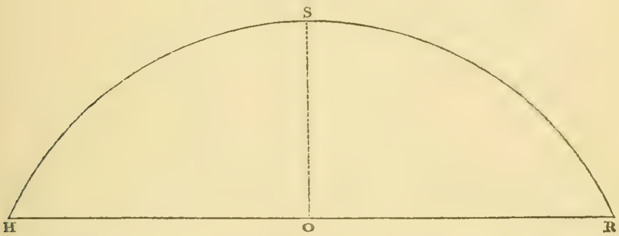
the sphere of the heavens lines that of course cannot be drawn there.

The first problem that we shall submit to the reader refers to the apparent course of the sun in the heavens, from its rising to its setting on the 21st of March.

We will suppose that a person is situated at a locality in 52° north latitude, and facing the south. At 6 o'clock in the morning on the 21st of March the sun would rise above the eastern point of the horizon, trace a curve or arch in the heavens, until at the south it would attain an elevation above the horizon of 38°. When exactly south the sun would move nearly horizontally for a short part of its course; it would then gradually descend, slowly at first, but more rapidly afterwards, and at 6 o'clock P.M. would set below the western point of the horizon.

If we could mark out on the heavens the course traced by the sun, as stated above, we should draw a curve similar to H S R in the annexed diagram. In this diagram the horizon

FIG. I.



is represented by the straight line H O R, H being the east, O the south, and R the west points on the horizon. S would be the position of the sun relative to H O R when the sun was south, and the arc O S, representing the sun's altitude, would be 38°.

It will be seen that, if we could sketch or mark on the sky the course traced by the sun, this course would appear to us a great arch, as shown by H S R.

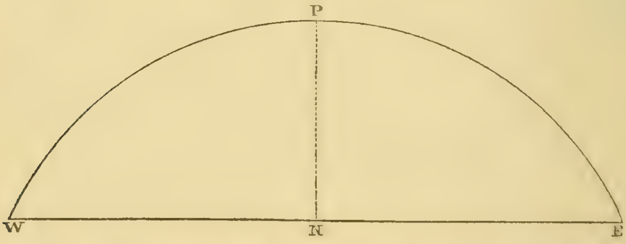
Now this arch traced by the sun on the 21st of March is the position which the earth's equator would occupy if produced to the distant sky, and this arch is termed the equinoctial. The equinoctial therefore cuts the east and west points of the horizon, traces a curve in the heavens, and at its south point is as many degrees above the horizon as 90° exceeds the latitude of the place of observation.

We will now suppose that the observer turns round and faces the north, and traces out on the sky the half of a great

circle which cuts the east and west points of the horizon, and passes through the pole of the heavens. This circle the reader, on reflection, will perceive must be a circle at right angles to the equinoctial, and its trace in the sky would be as follows :—Starting from the east point of the horizon, this circle would trace an arch in the heavens the highest part of which would be at the pole, when its altitude above the horizon would be equal in degrees and minutes to the latitude of the place of observation ; the curve would continue its arch-like form, and would cut the western point of the horizon. This curve or arch traced in the sky by a great circle passing through the pole, and cutting the east and west parts of the horizon, would appear as shown in the following diagram.

In this diagram N represents the north portion of the horizon, P the pole of the heavens, E the east and W the west points on the horizon, whilst the curve E P W shows

FIG. 2.



the curve or arch traced by a great circle in the sky, which great circle is at right angles to the equinoctial, and cuts the east and west portions of the horizon.

The reader may perhaps wonder what these arches have to do with the pole star and the pointers : they have everything to do with them, and in fact are the key to this and to other mysterious problems which seem to have hitherto greatly perplexed some persons, for both this and the preceding arch are in reality straight lines, though they *appear* as curves to us ; for both this curve and the arch traced by the equinoctial would appear like straight lines to a person at the north pole of the earth, the equinoctial there appearing coincident with the horizon, and therefore like a straight line, the curve E P W appearing like a vertical line and as straight as a plumb-line.

We will now place relatively to the curved line W P E three stars, and show what would be the relative changes

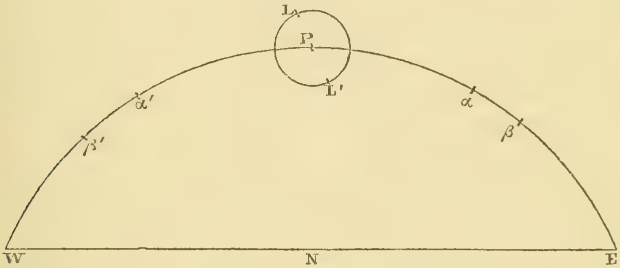
in twelve hours of these three stars, and due merely to the rotation of the earth on its axis.

W N E represents, as before, the horizon; the curved line P E a portion of the great circle, at right angles to the equinoctial, and cutting the east point on the horizon. P represents the pole of the heavens, and P W the remaining portion of the great circle that is above the horizon.

On the arc of the circle P E we represent two stars, α and β. A *straight* line joining these two stars, and produced in the direction of the pole, would reach a point L. We will suppose a star situated at L on the meridian, and above the pole.

Under the conditions given above the stars β and α point with great exactitude towards L, because an apparently straight line joining β and α, and produced, would pass through L.

FIG. 3.



In consequence of the rotation of the earth on its axis the star at L and above the pole would in twelve hours be carried round the true pole, P, and would trace a semicircle, and reach the point L′ *below* the pole, the arc P L being equal to P L′. Due to the same cause the stars α and β would appear to trace semicircles round P, and would occupy, in twelve hours from the time at which they were at α and β, the positions shown by α' and β'.

A line joining α' and β', and produced towards the pole, would now reach the point L, as before, but the star which was at L is now at L′, *below* the pole. Consequently the two stars α' and β' do not now point towards the same star at which they pointed when they were in the positions shown by α and β.

In order to illustrate the problem we have selected three stars in such a relative position as to show the effects in a prominent manner, and it happens that the actual position of the pole star and the pointers is not so very different from that of the three stars given above. We will now map out,

as it were, the pole star and the pointers according to their true position relative to one another, and as seen by an observer in N. latitude 52°, at various times.

The pole star is not situated exactly at the pole of the heavens, but is distant about 1° 27′ from the true pole, and therefore describes every twenty-four hours a circle round the pole, the radius of which circle is about 1° 27′,—that is, nearly three times the apparent angular diameter of the sun, for the diameter of the sun subtends an angle of about 32′.

The pointer nearest the pole is about 27° 34′ from the pole, whilst the second pointer is about 32° 49′ from the pole. These two stars during every twenty-four hours appear to trace circles in the heavens round the pole of the heavens, the radius of each circle being 27° 34′ and 32° 49′.

We will next refer to the lateral divergence of these three stars from what we may term a straight line, and in order to do this we must refer to what is called "*Right Ascension;*" a term which probably some readers may not be acquainted with, but we will give such a popular description thereof as shall render the explanation intelligible to any person.

The right ascension of the pole star is about one hour and twelve minutes, which converted into degrees is 18°. The right ascension of the nearest of the pointers, viz., α Ursæ Majoris, is ten hours fifty-five minutes, whilst the second pointer β Ursæ Majoris has a right ascension of ten hours fifty-three minutes. Converting these differences of right ascension into degrees it follows that the difference in right ascension between the pole star and the pointers is, in round numbers, 145° 43′.

Let us now explain this fact in more popular language.

If in the following diagram P represent the pole of the heavens, S the position of the pole star distant 1° 27′ from P, α and β the two pointers, then the angle S P α will be 145° 43′.

Fig. 4.

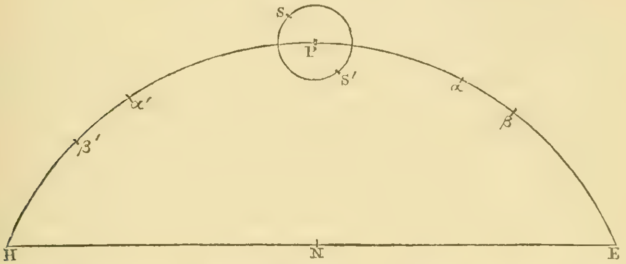
Now the point P always remains fixed in the heavens, and whilst S revolves round P, and α and β also revolve round P, yet the angle S P α will always remain a constant in value, and will always be 145° 43′.

We can now trace out the changes which occur in the *apparent* relative positions of the pointers and the pole star

under certain conditions, and we will first describe these when the pointers are east of the pole star.

In the following diagram P represents the pole, S the pole star, α and β the pointers, H N E the horizon, E the east, N

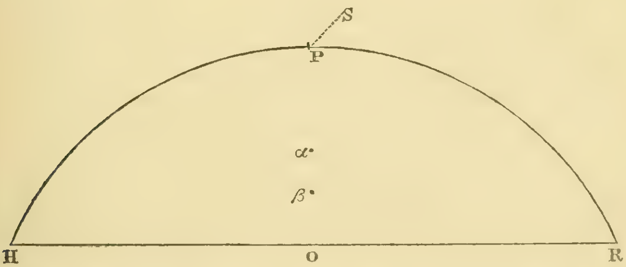
FIG. 5.



the north, and H the west points. Under these conditions the pointers point with tolerable exactitude towards the pole star. When twelve hours have elapsed the pole star will be seen at S′ in its circle, the stars α and β will appear in the positions α', β', and it is now evident that these two stars do not point at the pole star, although as before the angle S′ P α' is 145° 43′.

In six hours after the stars α' and β' were in the position shown in the last diagram they would be on the meridian and below the pole, and as these two stars differ only two minutes in right ascension they would, when on the meridian and below the pole, point almost vertically upwards. The pole star, however, would now be 90° in its circle from S′ in the last diagram, consequently the pole star and the pointers would as regards each other then occupy the relative positions shown in the following diagram, where H O R

FIG. 6.



represents the horizon, O the north point of the horizon, P the pole of the heavens, S the pole star, α and β the pointers. The angle S P α being as before 145° 43′.

It is evident that under the above conditions the pole star is not pointed at directly by the pointers, nor will it be until the stars α and β ascend to the east and reach nearly the same altitude as the pole star.

It will be evident from the preceding demonstrations that the pointers point with the least exactitude towards the pole star when then they are west of that star and on the great circle nearly at right angles to the meridian. It may appear somewhat singular to many readers when we state that this *appearance* (for it is but an appearance) will hold good for our latitudes, but it would not occur to a person who might be situated at the North Pole. Such a statement may seem incorrect, but it is a truth, the reason for which may be understood by the following description:—

To a person situated at the North Pole every star in the heavens would appear to trace a circle during twenty-four hours round a point exactly over his head. Every star, therefore, would appear to move parallel to the horizon. If these four or five stars were arranged in a straight line from the horizon up towards the zenith, these four or five stars would always appear to an observer at the pole to lie in the same straight line. The curve or arch joining P and R in the last diagram would appear to an observer at the pole a *straight* line rising from the horizon directly to the point over his head, and not a curve or arch, as it appears to a person in such a latitude as 52°.

Here, then, we have the key to the peculiar fact that the pointers do not always appear to us to point with equal accuracy towards the pole star. It is because owing to what we may term the peculiar perspective of the sphere of the heavens that which appears under one condition as a straight line may appear under another condition as a curved line, and as the pointers are on the apparent sphere of the heavens, and alter their relative positions as regards the horizon, the effects are such as we have stated them to be.

In order to render this problem as intelligible as it should be, we will again refer to the course or trace of the equinoctial on the sphere of the heavens, and described in an early page of this article. We pointed out that the equinoctial, if it could be marked out on the sphere of the heavens, would be represented by a great arch which cut the east and west points of the horizon, and attained an altitude at the south equal to 90° less the latitude of the place of observation. This curve would remain constantly marked on the sky during any number of rotations. It would be evident to our senses that a line must be a curved line which could be

drawn on the sky commencing from the east part of the horizon, rising rapidly at first as an arch does rise, then as the equinoctial approached the south the arch would be traced nearly horizontally, then it would slowly descend, afterwards more rapidly, and finally cut the west point of the horizon. Therefore every portion of this equinoctial would appear a portion of a curve, and it could not be considered a straight line any more than a rainbow appears a straight line.

What shall we say, however, when we trace out the equinoctial on the sky as it would appear to a person at the North Pole?

To an observer at the North Pole the terms east and west do not exist. The south is beneath his feet, the north exactly over his head, and the east and west undefined. To him the equinoctial would not rise above his horizon as it would to an observer in middle latitudes, but the equinoctial would *coincide* with his horizon in all directions.

To every person who may be on the same level as the sea, and when a clear defined sea outline is visible, the horizon appears like a *straight* line, and all parts of the horizon appear like portions of a straight line: therefore to an observer at the Pole the equinoctial will appear like a straight line, and will be coincident with the horizon, yet to an observer in middle latitudes the equinoctial will always appear as an arch in the heavens.

The same geometrical or optical laws which cause the equinoctial to appear a straight or a curved line, according as the observer is in one part or another part of the earth, also cause the pointers to appear under certain conditions to point exactly towards the pole star, whilst under other conditions they will not appear to point with equal accuracy to the same object.

The same laws produce other effects in connection with certain celestial bodies, some of which effects are so palpable as to attract the attention of every observer, whilst other phenomena are noticed (spontaneously as we may term it) only by those who possess good observational powers. When, however, the peculiar facts are pointed out, and the celestial objects themselves illustrate the problem, few persons are incapable of perceiving the paradox, and the majority are anxious to learn the causes which produced it.

In the present article we have confined our description to the pole star and the pointers, but on a future occasion we purpose treating two other phenomena even more easily observed than is the fact connected with the pointers; yet

these others have long remained mysteries to certain individuals.

The reader who has read and mastered the preceding problem may amuse himself by testing the observational power of his friends. He may enquire whether his friend has ever remarked the pole star and the pointers. Then he should carefully word his next question somewhat in the following terms:—"Of course the pointers always point with equal accuracy towards the pole star," and in the majority of cases he will obtain for his answer, "Of course they must do so, as the stars never alter their relative positions from each other." If the reader, instead of the above careful question, were to say, "Have you ever remarked that the pointers do not always point with equal accuracy to the pole star," he would in many cases receive some such answer as the following:—"Well, I fancy I have noticed it, but I am not very sure about it."

So rarely do we find any but the most candid and progressive, who are willing to acknowledge their ignorance of a subject, that the above problem may afford those who have thoroughly mastered it considerable amusement and some information when they cross-examine those among their acquaintances who profess to have a knowledge of astronomy.

II. PEAT BOGS.

By G. H. Kinahan, M.R.I.A., &c.

THE formation or growth of peat bogs and that of most, at least, of our principal coal seams, were evidently very similar: it is therefore interesting to be acquainted with what may be ascertained about the former.

Why peat bogs began to grow at first is a question often asked, but as yet not satisfactorily answered. In Great Britain and Ireland there are three classes of peat, distinguished by three modes of occurrence, namely, *first*, the preglacial or intraglacial peat; *second*, submarine peat; and *third*, the subaërial, or the peat accumulations that are still being formed. The latter, however, as will hereafter be mentioned, can in certain places be subdivided, as there is proof of a long cessation of peat growth having intervened over vast areas during its period of formation. In this paper

the facts witnessed in Ireland will be those chiefly considered, but at the same time some reference will be made to places in Scotland or England as they show the similarity of the growth of peat in the three countries.

There does not appear to be in Ireland any peat accumulations under the undoubted normal boulder-clay-drift. Near Nenagh, Co. Tipperary, there is a black peaty bed under "forty-three feet of hard calcareous clay, with numerous lumps of limestone intermixed, but unstratified."* In Boleyneendorrish Valley, near Gort, Co. Galway, there is an argillous peat under about 25 feet of a glacial drift,† and in some of the pits of the Newtown Colliery, Queen's Co., 3 feet of peat was found under 96 feet of drift which was, at least in part, glacial. From this it will be seen that although such peaty accumulations may not have been formed prior to the glacial period, yet that some are evidently intraglacial,—that is, they were formed subsequent to the beginning and prior to the end of the glacial period. There are also fragments of peat found in the marine gravels in the country west of the south portion of Lough Corrib, Co. Galway. These gravels, when compared with the gravels of the "Esker Sea period" of the central plain of Ireland, seem to be evidently recent; still they appear to have been formed before the final disappearance of the ice from the neighbouring highlands, as ice-carried erratics are occasionally found lying upon them. In the peaty accumulations of Boleyneendorrish, Dr. Melville, of the Queen's University and Queen's College, Galway, detected numerous cones of the *Pinus sylvestris*, cones of the *Abies excelsa*, De C., fragments of wood (chiefly coniferous), portions of branches, scales of bark, pieces of fir bark, an imperfect hazel nut, and leaves of plants, which prove the presence of such trees on the western portion of the British Islands during at least the latter portion of glacial times.

Submarine peat is not uncommon off the coast of Ireland, and in many of the estuaries and brackish water lagoons. In the east of Ireland, at the present time, there is very little peat growing on the lowlands, yet off the coast, and in the estuaries, &c., submerged bogs are not uncommon. They have been proved to occur from high-water mark to a depth of 20 or 30 feet; those under the estuary muds of Wexford Harbour having been found to a depth of 20 feet below the surface of the mud, or about 23 or 24 feet below

* Paper by T. Oldham, M.R.I.A., Journ. Geol. Soc. Dubl., vol. iii., p. 64.
† Mem. Geol. Survey, *ex* Sheets 115 and 116, p. 28.

mean high-water mark: these bogs are found to contain the roots and trunks of oak, yew, deal, hazel, sallow, and a timber like ash, also hazel nuts. In the south and west of Ireland lowland bogs are numerous, and it is a common occurrence to find—off a coast where bogs now form the sea margin—peat from 8 to 12 feet deep, even at low water of spring tides, giving a depth below mean high-water mark of from 23 to 25 feet. In these bogs the roots and trunks of oak, yew, deal, willow, and hazel are found, similar to the tree remains that occur in the inland bogs. Off the south-east coast of England there are also submarine bogs; those in Romney Marsh and Pevensey Level being at nearly similar depths below the mean high-water mark. It has been suggested that the peat accumulations found in lagoons, estuaries, and even on the open coast, may have grown at their present levels, the sea being kept out from them by a barrier of sand, gravel, or the like, which was subsequently swept away or moved inland. If, however, we consider a moment, the erroneousness of such an idea is apparent. Take, for instance, such places as Romney Marsh in England, or Wexford Estuary in Ireland, where we find the roots of oak *in situ* more than 15 feet below the mean low-water mark. These trees, at the time they were growing, would have been liable at any time to have been inundated, while even at low water of spring tides they would have had no drainage. Such trees as the willow and alder might possibly grow under these conditions; but such trees as the oak, yew, fir, ash, and hazel require a drainage from the ground on which they grow; these, then, never could grow and arrive at maturity on ground below low-water mark. Such bogs, therefore, as those in which the remains of the last-mentioned trees are found, must have been while the trees were growing above high-water mark. Moreover, the process of subsidence would seem to have been gradual, to allow the peat-forming plants time to grow and decay in oft-repeated succession.

The plant-remains in the submarine peat are, as a general rule, similar to those found in the subaërial lowland or "red bogs," and we may reasonably conclude that the peat accumulated under very similar conditions,—that is, as a "red bog," and not in a lagoon or marsh. It also must have taken a very long time to accumulate, as peat when drained will contract more than half its height, while if weighted and compressed—as the peat under estuary mud—it would be reduced a third or fourth more, so that the 5 feet of peat under the muds of Wexford Estuary would represent a growing bog of from 15 to 20 feet in depth.

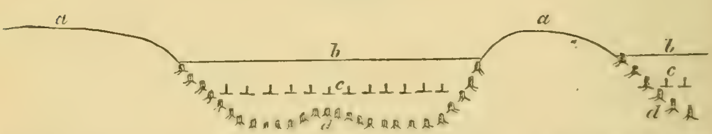
The normal lowland or "red bogs" of the central plain of Ireland are very similarly circumstanced to the ordinary coal seams, having an under-clay which is more or less penetrated by the roots of the trees and larger plants which at the first occupied the land. In most of them, as also in many other low-lying bogs, the roots and the trunks of the trees under the peat, or in the lowest strata, are principally those of oak and yew, as if prior to the growth of the peat the low country was for the most part a vast forest of these kinds of trees, the oak greatly preponderating. In these forests mosses and other peat-producing plants began to grow and flourish, till eventually they stopped the drainage, and formed an envelope of peat which gradually killed the trees, while subsequently the stems rotted off, between wind and water, the trunks toppling over and being entombed by the succeeding growth of the peat. After the disappearance of the major portion of the oak forest, the bogs for years gradually increased in depth, till suddenly—from some as yet unexplained cause—their growth ceased, and on their surfaces forests of deal sprang up. During all this time, however, portions of the original oak forest were preserved, and some of them apparently remain in certain places to the present day. This seems to have been due to the oaks and associated trees being destroyed only in those comparatively level places in which the peat could easily accumulate. On hills that were above the general level of the peat the oak would still flourish, and during the "Deal Forest Age" each of these hillocks remained an oak grove. At the present day, in many parts of Ireland, the hills and exposures of drift in a flat bog are called "*derries*" (*Anglicè*, oak-woods), the ancient name, which has survived down to the present age from the time when they were oak groves surrounded by forests of deal. Still this name may be more modern, as it is probable that, even after the deal forests disappeared, many of these hills still remained oak-woods. The derries in the midland counties of Ireland have long since been cleared of their timber, and are now under tillage or grass land, but in a few places—such as some of the wild tracts in Mayo—the oak may still be found growing on the drift islands in the bogs, it always being associated with yew, hazel, birch, ash, and holly; probably the last three trees were also denizens of the primary forests, but their timber has long since disappeared, they being of kinds that rot quickly in bog.

That such was the mode of the growth of the bogs is proved by the different sections exposed, as beneath the peat

in the clay, marl, or gravel, are found the "corkers," or roots of the oak and yew, following the undulations of the ground, while above them—in horizontal layers, and separated from them by from about 4 to 10 or 12 feet of peat—are the roots of the deal forest, which at the sides of the hills join the oak roots, as shown in the accompanying sketch section. In a few lowland bogs, however, and in many bogs in the mountainous districts, the lower and upper systems of corkers will belong to deal trees, as if in such places there had been two distinct ages of deal forests.

Some of the lowland or red bogs are of great depth, in a few places up even to 50 feet, but on an average they generally do not exceed 20 or 30 feet, and often are much less. A typical red bog gives four kinds of peat: near the surface is a clearing of more or less living organic matter, from 3 to 6 feet in thickness; under this is *white turf*, then *brown turf*, and lowest of all *black* or *stone turf*. White turf is a nearly pure organic substance, very light when dried, burns quickly, giving out only a little heat, and leaves little or no ash.

FIG. 7.



a. Drift Hills or Derries. *b.* Surface of bog. *c.* Deal corkers. *d.* Oak corkers on gravel.

Brown turf is always more or less mineralised. Black or stone turf is a chemico-organic production, and may contain such minerals as pyrite or marcasite; often it is semi-crystalline and seems to pass into *lignyte*, and when burnt it always leaves more or less ash. A variety in many bogs is locally called "Monagay" turf, which is very brittle, and full of the fragments and stems of flagger-like plants. Under monagay turf marl always is found, while under typical black turf there is usually gravel, suggesting that the monagay turf accumulated in a marsh. Another variety, always found at the very bottom of a bog when cut, is of a pale greenish-yellow colour: this, if allowed to dry, fully "melts" or disintegrates under the atmosphere, but if stacked when half dry it becomes a beautiful, hard, compact turf, that burns with a strong heat and brilliant light.

The residue or ash of peat generally, but not always, is greater the more deeply the peat is seated. In some places mineral matter may be carried up into the peat from springs and the like; but the ash of peat seems usually due to the

plants growing on the surface collecting their inorganic food from the atmosphere, and after their decay to the collected mineral substances being continually carried downwards by the water percolating through the mass: in this way each lower portion becomes more impregnated than the parts above it, and when burnt its ash or residue is greater.

The bogs on mountains are vastly different from those on the low lands. The latter usually grow on more or less flat places, where the substratum is non-porous, on which they form extensive level plains, while in the mountainous districts bog may grow anywhere on the hill-tops, as well as in the valleys, and from the latter they creep up the slopes over porous and non-porous strata. They are generally of small depth as compared with the lowland bogs, as here the peat grows much more slowly and more densely. In typical mountain bogs the *clearing*, or partly living organic matter, rarely exceeds 12 inches in depth, and this lies on *brown turf*, under which there may be *black turf*, but not always, as in some places the peat envelope is too thin to form the latter by compression. The difference between mountain and lowland peat is evidently due to the different rate of growth of the peat-producing plants in the respective positions. In the low flat land the growth of the plants is more rapid than among or on the mountains; consequently while on the former a light spongy substance is accumulating, on the latter a heavy felt-like mass is being formed. Besides, in all mountainous districts portions of the peat are very much subject to denudation by rain, runlets, and wind, which carry particles of peat from one place to another, and, by depositing them on the surface of the growing peat, keep the surface even, and make the peat compact and firm. In this way, on the growing peat may be deposited a layer of derivate or sedimentary peat, which will afterwards become interstratified by a subsequent growth of the plants. In flat mountain bogs, and also in the lowland bogs, when the latter are situated at the base of a hill or along a river subject at times to extensive floods, layers of sand, gravel, silt, or the like, may be found in the peat, such foreign material having been deposited on the surface of the bog during freshets, while afterwards the peat grows over them.

In coal-mining we find parts of seams absent; such deficiencies are called "faults," "troubles," "horses," &c., by the colliers, and the formations of these coal-less portions of a vein are explained by the study of some peat bogs; as we find the mass of the peat in some places denuded by the wind, and in others by a stream, which cut into it and break

its continuity, and if, after this, the bog were to be covered up by newer strata, such vacancies would be filled with foreign materials similar to the "troubles" or "horses" in a coal seam.

In the mountain bogs of the British islands the prevailing timber seems to be deal, and some of the sticks are of lengths that the fir rarely attains at the present day in these countries. The absence of the remains of the oak in the highlands of these islands would seem to suggest that the oak could not flourish above a certain altitude, which in Ireland seems to have been about the 400 feet contour line. This would seem to suggest that prior to the age of the first growth of the subaërial peat the lowlands, and the hill-sides to a certain height, were covered with oak forest, while above that height, the hills grew deal, the deal trees then flourishing at much greater altitudes than now, as large sticks are sometimes found in bogs at heights of above 1000 and 1200 feet.

From the above we may regard it as probable that there has been, since the glacial period at least, two ages of the most active growth of peat—*first*, after the great oak forest period, and *second*, subsequent to the deal forest period. That a considerable break occurred between them is evident, but the cause of it is unknown. There may indeed have been a third period, while the now submarine bogs were growing: this, however, does not appear probable, for at the present day the lowland bogs may grow at any height from high-water mark to the 200 or 250 contour line: it is therefore possible that the submarine bogs were forming at the same time as the lower strata of our present subaërial bogs, although now found under such different conditions. The turf-cutters on the sea shore generally tell you that "the turf is of the same depth at high- and low-water mark," but they cannot say if the corkers or roots of the trees stand perpendicular. If they are correct it would necessitate that seaward the peat has sunk more than at the shore-line. On a straight shore-line this might be possible, but on the indented coasts of our bays it is highly improbable.*

In studying bogs, one of the greatest difficulties is to procure reliable data by which to be enabled to estimate the rate at which they grow. Undrained bogs grow; drained

* Near the shore-line I have seen the corkers standing perpendicular, but I never saw an off-shore hole bottomed, as the incoming tide usually drives away the turf-cutters before they can take out all the turf; and I very much suspect my informants told me what they considered most probable, and not what they actually saw.

bogs, as long as the drains are effective, will not; and it would appear, from the Irish annals, that both before and since the English occupation attempts were made to drain and reclaim the lowland bogs, while they were again allowed to run wild in subsequent troublous times. These artificial stoppages of the growth of the peat have complicated matters, as respects some bogs, so that it is now impossible even to guess how long they may have been growing. At the present day the growth of the lowland bogs in Ireland is generally small, on account of their being more or less drained, and from turf being cut round their margins. In places, however, where the drainage has been for a long time neglected, a perceptible change in their height will take place: this, however, may in part be due to the bog soaking and swelling with water. A road through a bog will rise if the side drains are allowed to become choked. The margins of others, after the turf-cutting has been abandoned, gradually begin to resume their natural form, and if left long enough the peat will begin to creep out upon the adjoining upland, the growth being sometimes very rapid: certain bog-holes that were abandoned in 1848 are now nearly filled with new peat, but of a very soft spongy nature.

In the mountainous districts Nature has been less interfered with, and thus many facts in relation to the growth of bogs can be advantageously studied. Bogs naturally grow more readily on flats than elsewhere, and among the hills we find the deepest peat on the flat hill-tops and in the flat valleys: it, however, does not confine itself to such places, but creeps up and down the adjoining slopes. The latter process might be expected to be the easiest: this, however, does not appear to be the case, especially if the slope is steep. Bogs on an exposed hill are denuded at the edge during storms, especially if these are accompanied by rain. Such bogs are thus prevented from creeping downward, while the annual growth and decay of the plants increase their thickness, thus forming low long cliffs in all exposed places; it is only in sufficiently sheltered situations that the peat covering extends continuously downwards. But bogs creep rapidly upward, even in places where the slopes are composed of porous materials; as the wind, rain, and runlets will add boggy stuff to its edge, forming rapidly a soil in which heather, moss, and other peat-producing plants rapidly grow. In some of the valleys in the Connemara hills there are coarse shingle slopes, with a covering of peat often as much as 8 feet deep, while the bog on the adjoining flats is not much deeper. This peat originated with peaty matter

from the flat tops of the hills, that was lodged on the surface of the stones, by rain and wind, on which heather, &c., grew, and by the growth and decay of such the great thickness of peat was accumulated; from these shingles the peat is extending up the slopes of the hills.

In some of the once-inhabited but now depopulated valleys (and similar facts may be observed in parts of the highlands of Scotland) the growth of the peat is remarkable; it now having nearly obliterated the sites of the farmsteads and fields. The growth of peat differs on the hill-tops and in the valleys: on the first it is somewhat similar to that of lowland bog, in the plain it being principally due to the growth and decay of vegetation; for this reason it is more or less tussocky: but the turf in the valleys is only partially due to vegetable growth and decay, for it is partly derived from peaty matter carried on to it; it is therefore usually much more dense and level on the surface; it is, however, very variable in composition, as in some places it is more favourably situated than in others for receiving extraneous matter.

As yet no human relic has been recorded from the submarine bogs; but as such peat accumulations have only been proved by borings or small excavations, while vast extents are unexplored, it is not likely that they should have been found; but in the subaërial peats they are not uncommon. In Drumkelin bog, parish of Inver, Co. Donegal, a log house was found under 14 feet of bog, the house being 8 feet high, while *under* it was 15 feet of bog, in all 37 feet deep. Stone celts, sharpened stakes, and methers full of lard or butter, have at different times been exhumed from under bog from 10 to 15 feet deep, in the mountain pass N.W. of Glenbonniv, parish of Feakle, Co. Clare; and the late J. Beete Jukes, F.R.S., on seeing the huts, remarked that they were exactly similar to the huts built by the natives of Newfoundland to shelter them while waiting for the deer in their annual migration. In one place in the ridge of hills N.W. of the small market-town of Clifden, Co. Galway, wattle fences were found stuck in the clay under bog from 8 to 12 feet deep. The ridge of these hills is very uneven, large flat spaces occurring, usually connected by more or less narrow passes. These wattle fences consisted of stakes driven into the ground, and interwoven with horizontal rods or branches, gaps here and there being left open; they always occurred opposite to one of the passes leading from one flat to another; and what seems remarkable about them is that they are similar to the description of traps made for deer at the

present day in Russia. In the bog close to Castleconnell, Co. Limerick, a mether full of a substance like whey was found standing by an oak corker, and 5 feet above the oak corker was a layer of deal corkers, while a short distance above the horizon of the latter, extending from the edge of the bog next the old castle of the O'Connings to an esker called Goig, was a roadway of oak timber built similarly to an American corderoy, and over the roadway was from 10 to 12 feet of solid peat; alongside the road are said to have been the remains of ancient bog-holes in which the peat was cut in a mode similar to that of the present day. From this bog we learn that the oak forests were inhabited, while subsequently oak timber was accessible in the neighbourhood, probably on Goig, after the destruction—at least in part—of the great pine forests, and that peat fuel was used at a very early age. The finds of stone and other weapons, also of butter or lard, are too numerous to mention, the latter occurring sometimes in lumps without the trace of any envelope,—sometimes in methers, barrels, cloths, and in conical vessels made of hoops and straight sticks, lined with cloth; some of them evidently were purposely buried, while others seem to have been dropped, and subsequently covered by the growth of the peat.

Hitherto we have been occupied with facts; now, however, we will, in part, have to deal with conjectures. The intraglacial peats in the localities that have been enumerated seem to be very ancient; those at Newtown, Queen's Co., and near Nenagh, Co. Tipperary, undoubtedly are so, and after their accumulation must for years have been under an ice-sheet. The peaty stuff in the Boleyneendorrish Valley, Co. Galway, must also have been under ice, but probably at a much later age than the others, and possibly after the ice had left most of the low country, while the pieces of peat in the marine gravels west of Lough Corrib are—comparatively speaking—recent, as the gravels were formed subsequent to the Esker Sea period, and during the time when only the lands under the present 150 feet contour-line were submerged. In a coom that lies on the north side of Mount Leinster, Co. Carlow, there is a peat under drift from a few inches to over 4 feet in depth: this peat, however, has not been previously mentioned, as the drift evidently is not a normal glacial drift, but has been arranged by meteoric action, and was probably washed down from a higher part of the coom.

Since the oak forest age there must have been various changes in the climate of Ireland. Oaks, indeed, would

grow at the same altitudes as those at which the corkers are found in the bogs, but then it must be remembered that these places are probably at least 50 feet lower than they were when the oaks flourished; besides, the bog oak occurs in exposed situations in the west of the island, where now no trees of the same dimensions would grow; and deal at the present day cannot be grown at the altitudes or in the situations where large bog deal sticks are found.

The history of the submarine and estuary peats may be as follows:—When the land was about 40 or 50 feet higher than at present, or even more, forests grew; subsequently the drainage became defective, either from the land sinking or from the growth of ferns, mosses, and the like,—probably the latter,—till eventually the forests disappeared, and bogs replaced them. Afterwards the land must have been depressed, and probably in general rapidly, for if the sinking was gradual the upper strata of the bogs would have been formed from the growth and decay of flaggers, reeds, and other marsh plants, while usually the peat seems to have formed from the same class of vegetation as that which is now forming the upland bogs.

In the subaërial bogs the decay of the oak forest would be somewhat similar to the decay of the woods at the commencement of the submarine peats, as the growth and decay of ferns, mosses, and other denizens of a forest, would gradually stop the drainage, and produce bogs in all low-lying places; these eventually would extend their limits, until all the woods on the low lands were destroyed and covered by peat; but the stoppage of the growth of the bogs and the advent of the deal forest is more difficult to explain.

Deal trees could not be introduced or flourish on a bog until it was first drained and dried, and how this was accomplished can only be conjectured. It seems impossible that the drainage of the bogs could be due to artificial means, for although we know portions were reclaimed at different times, yet such reclamation could not have been universal, and in all parts of Ireland the pine forests seem to have existed at the same time; we are therefore forced to believe that they were introduced by natural causes. At the present day we have about five wet years, five of average character, five fine years, five of average character, and so on, in recurrent cycles; but a similar climate would not have furnished favourable conditions for the growth of the pine forests. We must therefore suppose that there was a long period of drought inauspicious to the growth of peat-producing plants, during which the bogs became drained, dried, and consoli-

dated; subsequently the fir trees grew upon them, and came to maturity. After the growth of the forests the trees would attract the rain, and the climate would again become moist, when peat-producing plants would flourish, till eventually the pines were also destroyed and replaced by bogs. Against such a theory it must be allowed that, after the forests were destroyed, the climate ought to have again become dry, yet there does not appear to have been any material change during the last two hundred years, since the destruction of the great upland forests in Ireland recorded in the annals.

In conclusion, we may attempt to make a rough estimate of the years that have passed since the beginning of the growth of the oak forest. Some of the largest oaks in the bogs have been calculated, by their rings, to be more than two hundred years old, while fir trees, by similar indications, are found to be over one hundred years old. Many bogs are a more or less felt-like mass, but in others each year's growth is represented by a layer or lamina, and these laminæ in the brown turf are usually from fifteen to twenty in number per inch, or about two hundred in a foot, while in the black turf the average is about four hundred in a foot. From these data the age of the oaks under the previously mentioned Castleconnell bog may be as follows:—

Oak forest age . . . . about	300	years.
Five feet black turf, at 400 years a foot . .	2000	,,
Time allowed for the change of climate, say	100	,,
Deal forest age . . . . about	200	,,
Twelve feet brown turf, at 200 years a foot .	2400	,,
	5000	

Such an estimate is evidently very low, as it ignores the white turf or clearing,—also the different artificial stoppages of the growth of the peat, one considerable stoppage, at least, being during the time the road to Goig was being made and used: we, however, learn that in this part of Ireland at least five thousand years must have elapsed since the oak first began to grow.

III. THE PAST HISTORY OF OUR MOON.

By Richard A. Proctor, B.A. (Camb.),
Author of "The Sun," "The Moon," &c.

THE appearance of two treatises upon the moon, both of them considerable in dimensions, and within six months of each other, indicates the renewed interest which astronomers are taking in the study of the nearest of all the celestial bodies. It is noteworthy, also, that in both these treatises,—in that by Nasmyth and Carpenter as well as in my own work,—the moon is regarded, not as a mere satellite of the earth, but as a planet, the least member of that family of five bodies circling within the asteroidal zone, to which astronomers have given the name of the terrestrial planets. There can be no question that this is the true position of the moon in the solar system. In fact, the fashion of regarding her as a mere attendant of our earth may be looked upon as the last relic of the old astronomy in which our earth figured as the fixed centre of the universe, and the body for whose sake all the celestial orbs were fashioned. In this aspect, also, the moon is a far more interesting object of research than when viewed as belonging to another and an inferior order. We are able to recognise in her appearances probably resulting from the relative smallness of her dimensions, and hence to derive probable information as to the condition of other orbs in the solar system which fall below the earth in point of size. Precisely as the study of the giant planets, Jupiter and Saturn, has led astronomers to infer that certain peculiarities must result from vastness of dimensions, so the study of the dwarf planets, Mars, our moon, and Mercury, may indicate the relations we are to associate with inferiority of size.

This thought immediately introduces us to another conception which causes us to regard with even greater interest the evidence afforded by the moon's present condition. It can scarcely be questioned that the size of any member of the solar system, or rather the quantity of matter in its orb, assigns, so to speak, the duration of that orb's existence, or rather of the various stages of that existence. The smaller body must cool more rapidly than the larger, and hence the various periods during which the former is fit for this or that purpose of planetary life (I speak with purposed vagueness here) are shorter than the corresponding periods in the life of the latter. Thus the sun, viewed in this way, is

the youngest member of the solar system, while the tiniest members of the asteroid family, if not the oldest in reality, are the oldest to which the telescope has introduced us. Jupiter and Saturn come next to the sun in youth; they are still passing through the earliest stages of planetary existence, even if we ought not rather to adopt that theory of their condition which regards them as subordinate suns, helping the central sun to support life on the satellites which circle around them. Uranus and Neptune are in a later stage, and perchance when telescopes have been constructed large enough to study these planets with advantage, we may learn something of that stage, interesting as being intermediate to the stages through which our earth and Venus on the one hand, and the giant brothers Jupiter and Saturn on the other, are at present passing. After our earth and Venus, which are probably at about the same stage of planetary development (though owing to the difference in their position they may not be equally adapted for the support of life) we come to Mars and Mercury, both of which must be regarded as in all probality much more advanced and in a sense more aged than the earth on which we live. In a similar sense,—even as an ephemeron is more aged after a few hours of existence than a man after as many years,—the small planet which we call "our moon" may be described as in the very decrepitude of planetary existence, nay (some prefer to think), as even absolutely dead, though its lifeless body still continues to advance upon its accustomed orbit, and to obey the law of universal attraction.

Considerations such as these give singular interest to the discussion of the past history of our moon, though they add to the difficulty of interpreting the problems she presents to us. For we have manifestly to differentiate between the effects due to the moon's relative smallness on the one hand, and those due to her great age on the other. If we could believe the moon to be an orb which simply represents the condition to which our earth will one day attain, we could study her peculiarities of appearance with some hope of understanding how they had been brought about, as well as of learning from such study the future history of our own earth. But clearly the moon has had another history than our earth. Her relative smallness has led to relations such as the earth never has presented and never will present. If our earth is, as astronomers and physicists believe, to grow dead and cold, all life perishing from her surface, it is tolerably clear from what we already know of her history that the appearance she will present in her decrepitude will be utterly

unlike that presented by the moon. Grant that after the lapse of enormous time-intervals the oceans now existing on the earth will be withdrawn beneath her solid crust, and even (which seems incredible) that at a more distant future the atmosphere now surrounding her will have become greatly reduced in quantity either by similar withdrawal or in any other manner, yet the surface of the earth would present few features of resemblance to that of the moon. Viewed from the distance at which we view the moon there would be few crateriform mountains indeed compared with those on the moon; those visible would be small by comparison with lunar craters even of medium dimensions; and the radiated regions seen on the moon's surface would have no discernible counterpart on the surface of the earth. The only features of resemblance, under the imagined conditions, would be probably the partially flat sea bottoms (though these would bear a different proportion to the more elevated regions) and the mountain ranges, the only terrestrial features of volcanic disturbance which would be relatively more important than their lunar counterparts.

I do not purpose, however, to discuss the probable future of the earth, having only indicated the differences just touched upon, in order to remind the reader at the outset that we have not in "the moon" a representation of the earth at any stage of her history. Other and different relations are presented for our consideration, although it may well be that by carefully discussing them we may learn somewhat respecting our earth, as also respecting the past history and future development of the solar system.

I have already, on two occasions, discussed in these pages some of the problems presented by the observed condition of the moon's surface, and in my treatise on the moon, I have in several places indicated the views towards which my study of the subject tended. But I have not attempted to present any general theory on the subject, feeling, indeed, that it was one which presented too many difficulties to be hastily dealt with. It seems to me, however, that the enunciation by Messrs. Nasmyth and Carpenter, of somewhat definite theories respecting the moon's surface, affords me a favourable opportunity for advancing considerations which I have had much in my thoughts during the last five or six months, and which appear to me to accord more satisfactorily with observed lunar appearances on the one hand as well as with known terrestrial and probable cosmical relations on the other, than the theories advanced in the treatise above referred to.

It appears reasonable to regard the moon, after her first formation as a distinct orb, as presenting the same general characteristics that we ascribe to our earth in its primary stage as a planet. In one respect the moon, even at that early stage, may have differed from the earth. I refer to its rotation, the correspondence between which and its revolution may probably have existed from the moon's first formation. But this would not materially have affected the relations with which we have to deal at present. We may apply, then, to the moon the arguments which have been applied to the discussion of the first stages of our earth's history.

Adopting this view, we see that at the first stage of its existence as an independent planet, the moon must have been an intensely heated gaseous globe, glowing with inherent light, and undergoing a process of condensation, "going on at first at the surface only, until by cooling it must have reached the point where the gaseous centre was exchanged for one of combined and liquefied matter." To apply now to the moon at this stage the description which Dr. Sterry Hunt gives of the earth:—"Here commences the chemistry of the moon. So long as the gaseous condition of the moon lasted, we may suppose the whole mass to have been homogeneous; but when the temperature became so reduced that the existence of chemical compounds at the centre became possible, those which were most stable at the elevated temperature then prevailing, would be first formed. Thus, for example, while compounds of oxygen with mercury, or even with hydrogen, could not exist, oxides of silicon, aluminium, calcium, magnesium, and iron, might be formed and condensed in a liquid form at the centre of the globe. By progressive cooling still other elements would be removed from the gaseous mass, which would form the atmosphere of the non-gaseous nucleus." "The processes of condensation and cooling having gone on until those elements which are not volatile in the heat of our ordinary furnaces were condensed into a liquid form, we may here inquire what would be the result on the mass of a further reduction of temperature. It is generally assumed that in the cooling of a liquid globe of mineral matter congelation would commence at the surface, as in the case of water; but water offers an exception to most other liquids, inasmuch as it is denser in the liquid than in the solid form. Hence, ice floats on water, and freezing water becomes covered with a layer of ice which protects the liquid below. Some metals and alloys resemble water in this respect. With regard to most other earthy

substances, and notably the various minerals and earthy compounds like those which may be supposed to have made up the mass of the molten globe, the case is entirely different. The numerous and detailed experiments of Charles Deville and those of Delesse, besides the earlier ones of Bischof, unite in showing that the density of fused rocks is much less than that of the crystalline products resulting from their slow cooling, these being, according to Deville, from one-seventh to one-sixteenth heavier than the fused mass, so that if formed at the surface they would, in obedience to the laws of gravity, tend to sink as soon as formed."

Here it has to be noted that possibly there existed a period (for our earth as well as for the moon) during which, notwithstanding the relations indicated by Dr. Hunt, the exterior portions of the moon were solid, while the interior remained liquid. A state of things corresponding to what we recognise as possible in the sun may have existed. For although undoubtedly any liquid matter forming in the sun sinks in obedience to the laws of gravity towards the centre, yet the greater heat which it encounters as it sinks must vapourise it, notwithstanding increasing pressure, so that it can only remain liquid near the region where rapid radiation allows of sufficient cooling to produce liquefaction. And in the same way we may conceive that the solidification taking place at any portion of the surface of the moon's or the earth's liquid globe, owing to rapid radiation of heat thence, although it might be followed immediately by the sinking of the solidified matter, would yet result in the continuance (rather than the existence) of a partially solid crust. For the sinking solid matter, though subjected to an increase of pressure (which, in the case of matter expanding on liquefaction, would favour solidification) would nevertheless, owing to the great increase of heat, become liquefied, and expanding would no longer be so much denser* than the liquid through which it was sinking as to continue to sink rapidly.

Nevertheless, it is clear that after a time the heat of the interior parts of the liquid mass would no longer suffice to liquefy the solid matter descending from the surface, and then would commence the process of aggregation at the centre described by Dr. Hunt. The matter forming the solid centre of the earth consists probably of metallic and metalloidal compounds of elements denser than those forming

* It would still be somewhat denser, because under the circumstances it would be somewhat cooler.

the known portions of the earth's crust.* In the case of the moon, whose mean density is very little greater than the mean density of the matter forming the earth's crust, we must assume that the matter forming the solid nucleus at that early stage was relatively less in amount, or else that we may attribute part of the difference to the comparatively small force with which lunar gravity operated during various stages of contraction and solidification.

In the case of the moon, as in that of the earth, before the last portions became solidified, there would exist a condition of imperfect liquidity, as conceived by Hopkins, "preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downwards. There would thus be enclosed between the inner and outer solid parts a portion of uncongealed matter," which may be supposed to have retained its liquid condition to a late period, and to have been the principal seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes, or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus.

Thus far we have had to deal with relations more or less involved in doubt. We have few means of forming a satisfactory opinion as to the order of the various changes to which, in the first stages of her existence as a planet, our moon was subject. Nor can we clearly define the nature of those changes. In these matters, as with the corresponding processes in our earth's case, there is much room for variety of opinion.

But few can doubt that, by whatever processes such condition may have been attained, the moon, when her surface began to form itself into its present appearance, consisted of a globe partially molten surrounded by a crust at least partially solidified. Some portions of the actual surface may have remained liquid or viscous later than others; but at length the time must have arrived when the radiating surface was almost wholly solid. It is from this stage that we have to trace the changes which have led to the present condition of the moon's surface.

It can scarcely be questioned that those seismologists are in the right who have maintained in recent times the theory

* It is thus, and not by the effects due to increasing pressure (effects which do not increase beyond a certain point) that we are to explain the fact that the earth's density as a whole is about twice the mean density of the matters which form its solid surface. It may be that this consideration, supported by the results of recent experimental researches, may give a significance hitherto not noted to the relatively small mean density of the moon.

that in the case of a cooling globe, such as the earth or moon at the stage just described, the crust would in the first place contract more quickly than the nucleus, while later the nucleus would contract more quickly than the crust. This amounts, in fact, to little more than the assertion that the process of heat radiation from the surface would be more rapid, and so last a shorter time than the process of conduction by which in the main the nucleus would part with its heat. The crust would part rapidly with its heat, contracting upon the nucleus; but the very rapidity (relative) of the process, by completing at an early stage the radiation of the greater portion of the heat originally belonging to the crust, would cause the subsequent radiation to be comparatively slow, while the conduction of heat from the nucleus to the crust would take place more rapidly, not only relatively but actually.

Now it is clear that the results accruing during the two stages into which we thus divide the cooling of the lunar globe would be markedly different. During the first stage forces of tension (tangential) would be called to play in the lunar crust; during the later stage the forces would be those of pressure.

Taking the earlier stage, during which the forces would be tensional, let us consider in what way these forces would operate.

At the beginning, when the crust would be comparatively thin, I conceive that the more general result of the rapid contraction of the crust would be the division of the crust into segments, by the formation of numerous fissures due to the lateral contraction of the thin crust. The molten matter in these fissures would film over rapidly, however, and all the time the crust would be growing thicker and thicker, until at length the formation of distinct segments would no longer be possible. The thickening crust, plastic in its lower strata, would now resist more effectively the tangential tensions, and when yielding would yield in a different manner. At this stage, in all probability, it was that processes such as those illustrated by Nasmyth's globe experiments took place, and that from time to time the crust yielded at particular points, which became the centres of systems of radiating fissures. Before proceeding, however, to consider the results of such processes, let it be noted that we have seen reason to believe that among the very earliest lunar formations would be rifts breaking the *ancient* surface of the lunar crust. I distinguish in this way the ancient surface from portions of surface whereof I shall presently have to speak as formed at a later time.

Now let us conceive the somewhat thickened crust contracting upon the partially fluid nucleus. If the crust were tolerably uniform in strength and thickness we should expect to find it yielding (when forced to yield) at many points, distributed somewhat uniformly over its extent. But this would not be the case if—as we might for many reasons expect—the crust were wanting in uniformity. There would be regions where the crust would be more plastic, and so readier to yield to the tangential tensions. Towards such portions of the crust the liquid matter within would tend, because there alone would room exist for it. The down-drawing, or rather in-drawing, crust elsewhere would force away the liquid matter beneath, towards such regions of less resistance, which would thus remain at (and be partly forced to) a higher level. At length, however, the increasing tensions thus resulting would have their natural effect; the crust would break open at the middle of the raised region, and in radiating rifts, and the molten matter would find vent through the rifts as well as at the central opening. The matter so extruded, being liquid, would spread, so that—though the radiating nature of the rifts would still be indicated by the position of the extruded matter—there would be no abrupt changes of level. It is clear, also, that so soon as the outlet had been formed the long and slowly sloping sides of the region of elevation would gradually sink, pressing the liquid matter below towards the centre of outlet, whence it would continue to pour out so long as this process of contraction continued. All round the borders of the aperture the crust would be melted, and would continue plastic long after the matter which had filled the fissures and flowed out through them had solidified. Thus there would be formed a wide circular orifice, which would from the beginning be considerably above the mean level of the moon's surface, because of the manner in which the liquid matter within had been gathered there by the pressure of the surrounding slopes.*

* I have occasion to make some remarks at this stage to avoid possible and (my experience has shown me) not altogether improbable misconception, or even misrepresentation. The theory enunciated above will be regarded by some, who may have read a certain review of my Treatise on the Moon, as totally different from what I have advocated in that work, and, furthermore, as a theory which I have borrowed from the aforesaid review. I should not be particularly concerned if I had occasion to modify views I had formerly expressed, since I apprehend that every active student of science should hope, rather than dread, that as his work proceeds he would form new opinions. And again, I am not in the least anxious to claim priority as to the enunciation of any theory, conceiving that claims of the kind seem as a rule indicative of a singular poverty of intellect on the part of those who make them (as though, having given birth to one good thought, they had no hope of ever being

Moreover, around the orifice, the matter outflowing as the crust continued to contract would form a raised wall. Until the time came when the liquid nucleus began to contract more rapidly than the crust, the large crateriform orifice

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delivered of another). Accordingly I have never, on the one hand, found occasion—to the best of my recollection—to claim a disputed priority; nor, on the other, have I ever been unwilling to abandon a theory which I have formerly maintained on insufficient grounds. (I need only point to my article on "Mars," in the "Quarterly Journal of Science," for April, 1873, as an illustrative instance, since I there not only abandon a theory I had once regarded with favour, but advocate carefully the theory advanced against it.) But the present instance is a somewhat peculiar one. In the "Saturday Review" for March 28th there is a paper which discusses lunar phenomena with considerable acumen. Of course there is the giant-making process which in the "Saturday Review" always precedes the process of giant-killing. The work of Messrs. Nasmyth and Carpenter is very warmly praised as a preliminary to the annihilation of their fundamental hypothesis. "We honour," says the condescending reviewer, "the courage which is daunted by no difficulty, and we feel that the authors were bound to make their theory a complete one; but we should have not the less felt bound to point out the glaring absurdities of this hypothesis had not the more than diffident tone in which the authors themselves speak of it rendered such a proceeding unnecessary." I am treated somewhat differently. A theory which I touched on first in these pages—the theory, namely, that some of the lunar markings on the moon's surface may have been due to meteoric downfalls in the long past ages when that surface was plastic, and meteor flights were more important than now—is described as one which "Mr. Proctor would have us believe," although I said in so many words "that I should certainly not care to maintain that as the true theory;" and is then summarily dismissed as a *facetia*. But while I am thus credited with insistance on a theory which I merely sketched as one not to be altogether overlooked in discussing peculiarities as yet not satisfactorily interpreted, the reviewer wholly omits to mention that much earlier in my book I had advocated, in a much more definite manner, a view closely resembling (so far as it relates to the large craters) that which he himself advances as preferable to Nasmyth's. I quote in full the passage in which I indicate and advocate, briefly but clearly, the theory urged above. At page 255 of my work on the "Moon," after describing the radiations from Tycho and other craters, I proceed—"It appears to me impossible to refer these phenomena to any general cause but the reaction of the moon's interior overcoming the tension of the crust, and to this degree Nasmyth's theory seems correct; but it appears manifest, also, that the crust cannot have been fractured in the ordinary sense of the word. Since, however, it results from Mallet's investigations that the tension of the crust is called into play in the earlier stages of contraction, and its power to resist contraction in the later stages,—in other words, since the crust at first contracts faster than the nucleus, and afterwards not so fast as the nucleus,—we may assume that the radiating systems were formed in so early an era that the crust was plastic. And it seems reasonable to conclude that the outflowing matter would retain its liquid condition long enough (the crust itself being intensely hot) to spread widely,—a circumstance which would account at once for the breadth of many of the rays, and for the restoration of level to such a degree that no shadows are thrown. It appears probable, also, that not only (which is manifest) were the craters formed later which are seen around and upon the radiations, but that the central crater itself acquired its actual form long after the epoch when the rays were formed." It will be manifest that the method here indicated as that by which the central crater acquired its actual form long after the rays had been formed, could only be that which the reviewer has indicated in the following passage:—"Assuming that the moon was once covered by a crust of rock, under a portion of which

would be full to the brim (or nearly so), at all times, with occasional overflows; and as a writer who has recently adopted this theory has remarked—"We should ultimately have a large central lake of lava surrounded by a range of hills, terraced on the outside,—the lake filling up the space they enclosed."

The crust might burst in the manner here considered, at several places at the same—or nearly the same—time, the range of the radiating fissures depending on the extent of the underlying lakes of molten matter thus finding their outlet; or there might be a series of outbursts at widely separated intervals of time, and at different regions, gradually diminishing in extent as the crust gradually thickened and the molten matter beneath gradually became reduced in relative amount. Probably the latter view should be accepted, since if we consider the three systems of radiations from Copernicus, Aristarchus, and Kepler, which were manifestly not formed contemporaneously, but in the order in which their central craters have just been named, we see that their dimensions diminished as their date of formation was later. According to this view we should regard the radiating system from Tycho as the oldest of all these formations.

At this very early stage of the moon's history, then, we regard the moon as a somewhat deformed spheroid, the regions whence the radiations extended being the highest parts, and the regions farthest removed from the ray centres being the lowest.* To these lower regions whatever was liquid on the moon's surface would find its way. The down-flowing lava would not be included in this description, as being rather viscous than liquid; but if any water

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at least lay melted rock of the same nature, nothing is more natural than that the contraction of the crust should cause great overflows of lava, which would spread far and wide; the outside portions would cool, but those near the centre of disturbance would be kept at their original temperature, and the tendency would be (as is so often noticed in eruptions) to melt the already solidified rock with which it was in contact, and thus the orifice would become wider."

* Where several ray centres are near together, a region directly between two ray centres would be at a level intermediate between that of the ray centres and that of a region centrally placed within a triangle or quadrangle of ray centres; but the latter region might be at a higher level than another very far removed from the part where the ray centres were near together. For instance, the space in the middle of the triangle having Copernicus, Aristarchus, and Kepler at its angles (or more exactly between Milichius and Bessarion) is lower than the surface around Hortensius (between Copernicus and Kepler), but not so low as the Mare Imbrium, far away from the region of ray centres of which Copernicus, Aristarchus, and Kepler are the principal.

existed at that time it would occupy the depressed regions which at the present time are called Maria or Seas. It is a question of some interest, and one on which different opinions have been entertained, whether the moon at any stage of its existence had oceans and an atmosphere corresponding or even approaching in relative extent to those of the earth. It appears to me that, apart from all the other considerations which have been suggested in support of the view that the moon formerly had oceans and an atmosphere, it is exceedingly difficult to imagine how, under any circumstances, a globe so large as the moon could have been formed under conditions not altogether unlike, as we suppose, those under which the earth was formed (having a similar origin, and presumably constructed of the same elements), without having oceans and an atmosphere of considerable extent; the atmosphere would not consist of oxygen and nitrogen only or chiefly, any more than, in all probability, the primæval atmosphere of our own earth was so constituted. We may adopt some such view of the moon's atmosphere—*mutatis mutandis*—as Dr. Sterry Hunt has adopted respecting the ancient atmosphere of the earth. Hunt, it will be remembered, bases his opinion on the former condition of the earth by conceiving an intense heat applied to the earth as now existing, and inferring the chemical results. "To the chemist," he remarks, "it is evident that from such a process applied to our globe would result the oxidation of all carbonaceous matter; the conversion of all carbonates, chlorides, and sulphates into silicates; and the separation of the carbon, chlorine, and sulphur in the form of acid gases; which, with nitrogen, watery vapour, and an excess of oxygen, would form an exceedingly dense atmosphere. The resulting fused mass would contain all the bases as silicates, and would probably nearly resemble in composition certain furnace-slags or basic volcanic glasses. Such we may conceive to have been the nature of the primitive igneous rock, and such the composition of the primeval atmosphere, *which must have been one of very great density*." All this, with the single exception of the italicised remark, may be applied to the case of the moon. The lunar atmosphere would not probably be dense at that primeval time, even though constituted like the terrestrial atmosphere just described. It would perhaps have been as dense, or nearly so, as our present atmosphere. Accordingly condensation would take place at a temperature not far from the present boiling-point, and the lower levels of the half-cooled crust would be drenched with a heated solution of hydrochloric acid, whose

decomposing action would be rapid, though not aided—as in the case of our primeval earth—by an excessively high temperature. "The formation of the chlorides of the various bases and the separation of silica would go on until the affinities of the acid were satisfied." "At a later period the gradual combination of oxygen with sulphurous acid would eliminate this from the atmosphere in the form of sulphuric acid." "Carbonic acid would still be a large constituent of the atmosphere, but thenceforward (that is, after the separation of the compounds of sulphur and chlorine from the air) there would follow the conversion of the complex aluminous silicates, under the influence of carbonic acid and moisture, into a hydrated silicate of alumina or clay, while the separated lime, magnesia, and alkalies would be changed into bicarbonates, and conveyed to the sea in a state of solution."

It seems to me that it is necessary to adopt some such theory as to the former existence of lunar oceans, in order to explain some of the appearances presented by the so-called lunar seas. As regards the present absence of water we may adopt the theory of Frankland, that the lunar oceans have withdrawn beneath the crust as room was provided for them by the contraction of the nucleus. I think, indeed, that there are good grounds for looking with favour on the theory of Stanislas Meunier, according to which the oceans surrounding any planet—our own earth or Mars, for example—are gradually withdrawn from the surface to the interior. And in view of the enormous length of the time intervals required for such a process, we must consider that while the process was going on the lunar atmosphere would not only part completely with the compounds of sulphur, chlorine, and carbon, but would be even still further reduced by chemical processes acting with exceeding slowness, yet effectively in periods so enormous. But without insisting on this consideration, it is manifest that—with very reasonable assumptions as to the density of the lunar atmosphere in its original complex condition—what would remain after the removal of the chief portion by chemical processes, and after the withdrawal of another considerable portion along with the seas beneath the lunar crust, would be so inconsiderable in quantity as to accord satisfactorily with the evidence which demonstrates the exceeding tenuity of any lunar atmosphere at present existing.

These considerations introduce us to the second part of the moon's history,—that corresponding to the period when the nucleus was contracting more rapidly than the crust.

One of the first and most obvious effects of this more rapid nuclear contraction would be the lowering of the level of the molten matter, which up to this period had been kept up to, or nearly up to, the lips of the great ringed craters. If the subsidence could place intermittently there would result a terracing of the interior of the ringed elevation, such as we see in many lunar craters. Nor would there be any uniformity of level in the several crater floors thus formed, since the fluid lava would not form parts of a single fluid mass (in which case, of course, the level of the fluid surface would be everywhere the same), but would belong to independent fluid masses. Indeed it may be noticed that the very nature of the case requires us to adopt this view, since no other will account for the variety of level observed in the different lunar crater-floors. If these ceased to be liquid at different times, the independence of the fluid masses is by that very fact established; and if they ceased to be liquid at the same time, they must have been independent, since, if communication had existed between them, they would have shown the uniformity of surface which the laws of hydrostatics require.*

The next effect which would follow from the gradual retreat of the nucleus from the crust (setting aside the withdrawal of lunar seas) would be the formation of corrugations, —in other words, of mountain-ranges. Mallet describes the formation of mountain-chains as belonging to the period when "the continually increasing thickness of the crust remained such that it was still as a whole flexible enough, or opposed sufficient resistance of crushing to admit of the uprise of mountain-chains by resolved tangential pressures." Applying this to the case of the moon, I think it is clear that—with her much smaller orb and comparatively rapid rate of cooling—the era of the formation of mountain-chains would be a short one, and that these would therefore form a less important characteristic of her surface than of the earth's. On the other hand, the period of volcanic activity which would follow that of chain-formation would be *relatively* long continued; for regarding this period as beginning when the thickness of the moon's crust had become too great to admit of adjustment by corrugation, the comparatively small pressure to which the whole mass of the moon had been subjected by lunar gravity, while it would on the one hand cause the period to have an earlier commencement

* It is important to notice that we may derive from these considerations an argument as to the condition of the fluid matter now existing beneath the solid crust of the earth.

(relatively), and on the other would leave greater play to the effects of contraction. Thus we can understand why the signs of volcanic action, as distinguished from the action to which mountain-ranges are due, should be far more numerous and important on the moon than on the earth.

I do not, however, in this place enter specially into the consideration of the moon's stage of volcanic activity, because already, in the pages of my Treatise on the Moon (Chapter VI.) I have given a full account of that portion of my present subject. I may make a few remarks, however, on the theory respecting lunar craters touched on in my work on "The Moon." I have mentioned the possibility that some among the enormous number of ring-shaped depressions which are seen on the moon's surface may have been the result of meteoric downfalls in long past ages of the moon's history. One or two critics have spoken of this view as though it were too fantastic for serious consideration. Now, though I threw out the opinion merely as a suggestion, distinctly stating that I should not care to maintain it as a theory, and although my own opinion is unfavourable to the supposition that any of the more considerable lunar markings can be explained in the suggested way, yet it is necessary to notice that on the general question whether the moon's surface has been marked or not by meteoric downfalls scarcely any reasonable doubts can be entertained. For, first, we can scarcely question that the moon's surface was for long ages plastic, and though we may not assign to this period nearly so great a length (350 millions of years) as Tyndall—following Bischoff—assigns to the period when our earth's surface was cooling from a temperature of 2000° C. to 200°, yet still it must have lasted millions of years; and, secondly, we cannot doubt that the process of meteoric downfall now going on is not a new thing, but, on the contrary, is rather the final stage of a process which once took place far more actively. Now Prof. Newton has estimated, by a fair estimate of observed facts, that each day on the average 400 millions of meteors fall, of all sizes down to the minutest discernible in a telescope, upon the earth's atmosphere, so that on the moon's unprotected globe—with its surface one-thirteenth of the earth's—about 30 millions fall each day, even at the present time. Of large meteoric masses only a few hundreds fall each year on the earth, and perhaps about a hundred on the moon; but still, even at the present rate of downfall, millions of large masses must have fallen on the moon during the time when her surface was plastic, while presumably a much larger number—including

many much larger masses—must have fallen during that period. Thus, not only without straining probabilities, but by taking only the most probable assumptions as to the past, we have arrived at a result which compels us to believe that the moon's surface has been very much marked by meteoric downfall, while it renders it by no means unlikely that a large proportion of the markings so left would be discernible under telescopic scrutiny; so that strong evidence exists in favour of that hypothesis which one or two writers (who presumably have not given great attention to the recent progress of meteoric astronomy) would dismiss "without consideration" (the way, doubtless, in which they have dismissed it).

I would, in conclusion, invite those who have the requisite leisure to a careful study of the distribution of various orders of lunar marking. It would be well if the moon's surface were isographically charted, and the distribution of the seas, mountain-ranges, and craters of different dimensions and character, of rills, radiating streaks, bright and dark regions, and so on, carefully compared *inter se*, with the object of determining whether the different parts of the moon's surface were probably brought to their present condition during earlier or later periods, and of interpreting also the significance of the moon's characteristic peculiarities. In this department of Astronomy, as in some others, the effectiveness of well-devised processes of charting has been hitherto overlooked.

IV. MODERN RESEARCHES IN TROPICAL ZOOLOGY.*

ORDINARY modern books of travel are justly ranked among the dreariest of literary productions. Their authors treat the countries which they visit merely as a stage for the display of their own imagined perfections, their omniscience, their courage, their skill as sportsmen, and above all, the importance of their mission to the distinguished personages with whom they became acquainted. To all this the work before us offers a complete and a delightful contrast. Mr. Belt does not obtrude his own personality upon us. Like all genuine men, he forgets "self" over his subject. Instead of informing us whether

* The Naturalist in Nicaragua; a Narrative of a Residence at the Gold Mines of Chontales. By THOMAS BELT, F.G.S. London: John Murray.

or no he received "the salary of an ambassador and the treatment of a gentleman," he scatters before us, broadcast, facts, interesting and novel; valuable hints for future research, and generalisations which will amply repay a close examination. Not alone the zoologist, the botanist, the geologist, but the antiquarian, the ethnologist, the social philosopher, and the meteorologist, will each find in these pages welcome additions to his store of knowledge and sound materials for study. With all this, the work is not a mere dry catalogue of facts, such as Henry Cavendish might have written: it is eminently a "readable book." Though without any effort at fine writing, the beautiful forest landscapes of Central America are brought vividly before us. For instances, we refer our readers to the work itself.

In his professional investigations of the gold deposits of Chontales, the author was struck with the scarcity of alluvial gold in the valleys, even in the neighbourhood of rich veins of auriferous quartz. This fact, reminding the observer of the similar scarcity in the valleys of Nova Scotia and North Wales, can be explained "on the supposition that the ice of the glacial period was not confined to extra-tropical lands, but, in Central America, covered all the higher ranges and descended to at least as low as the line of country now standing at two thousand feet above the sea, and probably much lower." That glaciers would have this effect is indubitable. As the author remarks: "When the denuding agent was water, the rocks were worn away, and the heavier gold left behind at the bottom of the alluvial deposits; but when the denuding agent was glacier-ice, the stony masses and their metallic contents were carried away, or mingled together in the unassorted moraines."

We may, at first sight, feel sceptical concerning the existence of glaciers in the low grounds within 13° of the equator. But the testimony of the rocks appears irresistible. "The evidences of glacial action were as clear as in any Welsh or Highland valley. There were the same rounded and smoothed masses of rock, the same moraine-like accumulations of unstratified sand and gravel, the same transported boulders that could be traced to their parent rocks, several miles distant." Glacial scratches were not, indeed, observed; but these are rarely detected on surfaces of rock exposed to the atmosphere. We must, also, remember that Professor Hart has found glacial drift extending from Patagonia all through Brazil to Pernambuco, whilst the late lamented Agassiz has observed glacial moraines up to the equator. On a former journey Mr. Belt discovered in

the province of Maranham, in Brazil, a great drift deposit apparently of glacial origin. Hence, he, very justifiably, thinks it highly probable that the ice deposits of the glacial period were far more extensive than has been generally supposed, existing at once in both the northern and southern hemispheres, and leaving, in America at least, only the lower lands of the tropics free from the icy covering.

Were the causes of such a reduction of the earth's general temperature cosmic or merely terrestrial? The author does not enter minutely into this question, though, with Professor Heer, he considers that the cold of the glacial period, like the warmth of the miocene epoch, "cannot be explained by any re-arrangement of the relative positions of land and water." It is very true that were the circum-polar lands, British North America and the Russian empire, deeply submerged beneath the ocean, and were a corresponding amount of land raised where now rolls the inter-tropical Pacific, the temperature of the world would be much ameliorated. But the very evidence of the heat of the miocene ages,—the existence, at that time of the beech, the hazel, and the plane in Spitzbergen, in north latitude 78°, proves that the polar regions were not an unbroken expanse of water. Similarly, the proofs of glacial action within the torrid zone show that during the glacial period land was by no means wanting between the tropics. It seems, therefore, highly probable, that in regarding these alternating epochs of heat and cold, "we are face to face with a problem whose solution must be attempted, and doubtless completed, by the astronomer."

But there is another phase of the subject to which the attention of the author is mainly given. Every botanist and geologist, on hearing of these enormous ice-deposits in what are now some of the most luxuriant climates of the world, will ask what must have become of all tropical forms of organic life? The very *tierras calientes* in those days, instead of towering palms and of epiphytal arads and orchids, must have possessed a vegetation little richer than that of England or Germany in the present order of things. Heliconii and Morphos can never have unfolded their delicate wings on the chill blast from the ice-deserts. The author meets this difficulty by the hypothesis that "a refuge was found for many species on lands now below the level of the ocean, but then uncovered by the lowering of the sea, consequent upon the immense quantity of water locked up in the form of ice, and piled upon the continents." A variety of evidence is adduced in support of this view. The distribution of animal life over small islands now

separated by shallow seas is readily explained on such supposition. Professor Hartt found reason to believe that during the time of the drift, Brazil stood at a much higher level than at present. Mr. Alfred Tylor considers that during the glacial epoch the level of the sea must have been reduced at least 600 feet, an estimate which our author extends to 1000. Mr. Wallace, in his work on the Malay Archipelago, infers from the distribution of animal life that Borneo, Java, and the more western islands of the group were at one time connected with each other, and with the main land of Asia, while New Guinea and the islands to the eastward were joined to Australia. The Cyclopean ruins found in certain islets of the Pacific, and utterly out of keeping with their present size and population, point in the same direction. Easter Island, as the author observes, could never have supported the race that reared such monuments. But if the present island was once one of a chain of hills overlooking wide and populous lowlands—now submerged beneath the blue waters of the Pacific—such remains become intelligible.

Mr. Belt's theory, as we may provisionally call it, furnishes the key to a number of ancient traditions on both sides of the Atlantic.

May not the Atlantis of Plato, of Theopompus, and of Proclus, finally swallowed up by the sea, have been "that great continent in the Atlantic, on which the present West Indian Islands were mountains?" The warlike and predatory character ascribed to its inhabitants by Greek and Egyptian story is in full harmony with their probably Carib origin. Mr. Belt's theory throws also a wonderful light upon the sagas of a universal deluge current, according to Catlin, among one hundred and twenty different tribes of North and South America. Those low-lying lands, if they existed at all, must have been the refuge during the glacial epoch, not merely of the principal forms of vegetable and animal life, but of the human race. Whatever civilisation existed must have had there its seat. Suppose, now, that the temperature of the earth experienced, from some unknown cosmical cause, a sudden elevation. The glaciers, or rather the ice-caps, resting on the present continents, are suddenly converted into torrents of water rushing down to the sea. The ocean level rapidly rises. Atlantis and all the other populous regions of the earth are engulphed. A hundred fathoms deep roll the waters over fields and cities, whilst a few only of the inhabitants escape in boats or find shelter on some lofty mountain.

Much, however, requires to be done, before such a theory can claim recognition among the established doctrines of science. Mr. Belt himself insists on the necessity of a careful and thorough going verification of his hypothesis. "When geologists have mapped out the limits of ancient glacier and continental ice all over the world, it will be possible to calculate the minimum amount of water that was abstracted from the sea; and if by that time hydrographers have shown on their charts the shoals and submerged banks that would be laid dry, fabled Atlantis will rise before our eyes between Europe and America, and in the Pacific the Malay Archipelago will give place to the Malay continent." In our opinion, a knowledge of the distribution of animal life, much more accurate and general than we now possess, will also be found needful for the full solution of the problem. Is it not also possible that by a careful examination of the less deeply submerged banks and shoals, evidence as to the presence or absence of traces of former human activity might be obtained?

We must not, however, forget that Mr. Belt's interesting hypothesis is not without a rival. The doctrine of areas of subsidence and elevation so ably expounded by Mr. Darwin in his Naturalists' Voyage, accounts for many of the same phenomena by an alteration of the level, not of the sea, but of the continents, and certainly agrees with many recognised facts. We have not space to examine in how far the two theories are necessarily antagonistic and mutually exclusive. It is plain that some of the Pacific Islands—which according to both views are the mountains of a now submerged continent—are still gradually subsiding. It is no less manifest that other portions of the earth's surface, *e.g.*, certain parts of the South American coast, are gradually rising. The evidence of both these progressive variations of level may be found in the above-mentioned work of Mr. Darwin. The phenomena of the atolls, likewise, appear to us to agree better with the assumption of a gradual subsidence of the dry land than with its inundation by the sudden influx of water. On the other hand, the Beltian hypothesis, harmonises best with the deluge-traditions of the old and the new world.

On Mr. Belt's view, we should naturally expect to find the fauna and the flora of any large island closely correspond to that of the adjacent continent, with which during the glacial period it would have been connected. Yet to this rule—if rule it be—there are notable exceptions of which account must be taken. Why should the larger Antilles be

free from carnivora? Cuba and Haiti would have afforded, in their glens and woods, ample and congenial lurking-grounds for the jaguar and the puma. Nor, to our knowledge, have these islands ever been so populous and so civilised that such unwelcome inmates would have been extirpated. Ceylon is separated from the mainland of India by a shallow sea. If the general level of the waters were reduced by 1000, or even by 600 feet, the space between the island and the continent would be bridged over. Yet, on the authority of Sir E. Tennant, there are more points of disagreement than of resemblance between the respective animal and vegetable forms on both sides of the straits. We are not aware of the depth of the Channel of the Mozambique. But scarcely could two richly developed faunas differ more strikingly than those of Madagascar and of South-eastern Africa. The monkeys of the continent are, in the island, replaced by lemurs. The cats and the gazelles in which Africa abounds are, we believe, totally wanting in Madagascar. The insect-world in particular shows a striking divergence. Madagascar is remarkably rich in those "animated jewels," the Cetoniadæ; but the species, and for the most part the genera, are distinct from those of Southern Africa, and, where not peculiar, remind us rather of forms developed in the Malay Archipelago.

But Mr. Belt's work is so replete with interest that we must, unwillingly indeed, desist from any further scrutiny of his geological speculations. His instances of the intelligence of ants are highly instructive:—"One day when watching a small column of these ants (*Eciton hamata*) I placed a little stone on one of them to secure it. The next that approached, as soon as it discovered its situation, ran backwards in an agitated manner and soon communicated the intelligence to the others. They rushed to the rescue; some bit at the stone and tried to move it, others seized the prisoner by the legs and tugged with such force that I thought the legs would be pulled off, but they persevered till they got the captive free. I next covered one up with a piece of clay, leaving only the ends of its antennæ projecting. It was soon discovered by its fellows, which set to work immediately, and by biting off pieces of the clay, soon liberated it. Another time I found a very few of them passing along at intervals. I confined one of these under a piece of clay, at a little distance from the line, with its head projecting. At last an ant discovered it and tried to pull it out, but could not. It immediately set off at a great rate, and I thought it had deserted its comrade, but it had only

gone for assistance, for in a short time about a dozen ants came hurrying up, evidently fully informed of the circumstances of the case, for they made directly for their imprisoned comrade and soon set him free. I do not see how this action could be instinctive. It was sympathetic help, such as man only among the higher mammalia shows."

We need not feel surprised if an observer accustomed to scrutinise the animal world so closely feels sceptical on the subject of "instinct." That notion is all very well for *literateurs*, lawyers, divines, or poets, who dabble a little in zoology, or for mere closet book-worms, who only catch a faint reflection of facts. But the naturalist of the field and the forest, though he recognises *instincts* in the lower animals, as in man himself, can but smile at "*instinct*," viewed as a mysterious entity, antithetically opposed to reason and supposed to act as its substitute in "our poor relations." The mutual helpfulness of ants is the more significant since, amongst social mammalia and birds, the tribe generally attack and put to death any of their number that suffers from an accident. The author gives two more instances of reason in ants. "I once saw a wide column trying to pass along a crumbling, nearly perpendicular slope. They would have got very slowly over and many of them would have fallen, but a number having secured their hold and reaching to each other, remained stationary, and over them the main column passed. Another time they were crossing a water-course along a small branch, not thicker than a goose-quill. They widened this natural bridge to three times its width by a number of ants clinging to it and to each other on each side, over which the column passed three or four abreast; whereas, excepting for this expedient, they would have had to pass over in single file and triple the time would have been consumed." In face of such facts the author asks—"Can it be contended that such insects are not able to determine by reasoning powers which is the best way of doing a thing, or that their actions are not guided by thought and reflection?" This view is much strengthened by the fact that the cerebral ganglia are more developed in ants than in any other insect, and that in all the Hymenoptera, at the head of which they stand, they are many times larger than in the less intelligent orders, such as beetles, or, we might add, than in those short-lived emblems of immortality, the butterflies.

Again, "amongst the ants of Central America, I place the *Eciton* as the first in intelligence, and as such, at the head of the Articulata. Wasps and bees come next, and

then others of the Hymenoptera. Between ants and the lower forms of insects there is a greater difference in reasoning powers than there is between man and the lowest mammalian. A recent writer (Houzeau) has augured that of all animals ants approach nearest to man in their social condition. Perhaps if we could learn their wonderful language we should find that even in their mental condition they also rank next to humanity."

As regards their social condition ants differ from us in having successfully established communism. At the present day all the social Hymenoptera must be viewed with intense interest on account of their working-order or neuters. These, as is well known, are females whose normal development has been checked. Are we to assume that "once upon a time" a woman's rights movement sprung up in bee-hives and ant-hills, which ended in reducing the males to a very unimportant position, and in limiting the number of the fully developed females? Are we to expect that the "strong minded" ladies who are now arising among us are the forerunners of a "neuter" order, and the heralds of a corresponding change in human society?

"The Hymenoptera standing at the head of the Articulata," resumes our author, "and the Mammalia at the head of the Vertebrata, it is curious to mark how in zoological history the appearance and development of these two orders (culminating, the one in the ants, and the other in the primates) run parallel. The Hymenoptera and the Mammalia both make their first appearance early in the secondary period, and it is not until the commencement of the tertiary epoch that ants and monkeys appear upon the scene."

Just as in a chain of mountains the second highest peak is often far remote from the culminating point, so it is in the upheaval of the animal world to reason. The position of a being in the zoological series, or its proximity in structure to man, bears no relation to its degree of intelligence.

Warm as is Mr. Belt's interest in ants in general, and the *Eciton* in particular, he does not overlook the fact that to man they are a great nuisance. Their habits of biting off the leaves of trees, and of fostering and defending such vermin as plant-lice, are, from our point of view, highly objectionable. It is, therefore, a fortunate circumstance that carbolic acid and corrosive sublimate afford us the means of putting a stop to their depredations. Mr. Belt's experiments with reference to this subject are well worth the attention of tropical agriculturists.

The great bulk of our author's observations on animals and plants have been made, as the title-page informs us, "in reference to the theory of evolution of living forms,"—in other words, to the views popularly included under the name Darwinism. These views can scarcely be said to have met with fair treatment. They have been dolefully groaned over by certain "grave and reverend," if not very "potent seigniors," on account of their supposed "questionable tendencies," almost in the very words applied in the days of Galileo to the heliocentric theory in astronomy. They have been sneered at by witling gallants by whom the hint of kinship with monkeys was felt as an "owre true joke." They have been "gushed" over in the daily press by leader writers and special correspondents whose conceptions of the subject were of the haziest. The hostile critics of the evolution theory have been, for the most part, not naturalists but outsiders, incapabable of entering into the full meaning of the evidence. To take a case strictly analogous, what should we think of a lawyer who in a difficult and complicated matter should give a professional opinion founded on documents drawn up in a language with which he had but a very slight acquaintance? "Theories," says M. Dumas, "are like crutches; to judge of their value we must take them and try to walk with them." This is what Mr. Belt does, and all who candidly read his book must concede that the doctrine of evolution throws a welcome light upon many abstruse phenomena, and brings into organic connection facts which previously lay scattered in hopeless confusion. Let us take for example the phenomenon of mimetism—an unhappily chosen name—of which our author gives some interesting instances.

A certain animal species is often found to show a superficial, but still striking and deceptive, resemblance to some other species, generally of a totally distinct group; to some vegetable product, or even to a fragment of lifeless matter. Instances of this are the "walking leaves," the moss insect, discovered and figured by the author, which closely simulates a few filaments of moss lying on the ground; the mimetic bug (*Spiniger luteicornis*), which in form, in the colouration of every part, and in its very movements mimics the hornet (*Priocnemis*). This simulation, of course involuntary, enables the insect either to elude its enemies or to secure its prey. Thus the bug above mentioned might fall an easy victim to many birds and insects which would hesitate before attacking a hornet. Hence the bug owes its safety to its resemblance to the hornet. There is, again, a small spider which closely

simulates a black, stinging ant. Spiders are greatly sought after and eagerly devoured by birds, whilst stinging ants are little relished. The Heliconii, a group of butterflies very largely developed in the West Indies and in Central America, have in all probability an offensive flavour. Birds, monkeys, and spiders avoid or reject them. There are other butterflies, belonging to perfectly distinct groups, which closely simulate the Heliconii, and thus escape the beaks of birds and the attention of spiders. The advocates of evolution explain this mimetism by supposing that, *e.g.*, a bug by some accidental variation from the normal structure of its congeners received a slight resemblance to a hornet. Its posterity, to whom this resemblance was transmitted, enjoyed an advantage in the struggle for existence over bugs which had remained true to the pristine type. Those of them in which the likeness to the hornet was most pronounced would, again, escape best from their enemies, and would thus have the greatest chance of becoming the progenitors of the next generation. The opponents of evolution have no way of accounting for mimetism beyond asserting that certain species were, for their protection, arbitrarily and *ab initio* endowed with these resemblances,—a most unsatisfactory explanation.

The same doctrine throws light on the difficulties often experienced in introducing foreign plants into any country. Thus the orange and the citron, in Central America, are very much more exposed to the attacks of *Ecitons* than any native vegetation. The reason is very simple:—"Through long ages the trees and the ants of tropical America have been modified together. Varieties of plants that arose unsuitable for the ants have had an immense advantage over others that were more suitable, and thus—through time—every indigenous tree that has survived has done so because it has had originally, or has acquired, some protection against the great destroyers. The leaf-cutting ants are confined to tropical America, and we can easily understand that trees and vegetables introduced from foreign lands, where these ants are unknown, could not have acquired—except accidentally and without reference to the ants—any protection against them."

If we reject the Darwinian view we should naturally infer that in any country foreign trees would enjoy an immunity from indigenous depredators, instead of being, as they are, especially singled out for attack. It is a curious fact that the leaf-cutting ants form, with the vegetable matter they thus carry off, manure-heaps, on which they cultivate minute fungi for the food of their larvæ.

With regard to the origin of species we extract the following interesting passage:—"The great mortality among the insects of Chontales, in 1872, has some bearing on the origin of species, for in times of such great epidemics we may suspect that the gradations that connect extreme forms of the same species may become extinct. Darwin has shown how very slight differences in the colour of the skin and hair are sometimes correlated with great immunity from certain diseases, from the action of some vegetable poisons and the attacks of certain parasites. Any variety of species of insects that could withstand better than others these great and probably periodical epidemics would certainly obtain a great advantage over those not so protected, and thus the survival of one form and the extinction of another might be brought about. We see two species of the same genus, as in many insects, differing but little from each other, yet quite distinct, and we ask why—if these have descended from one parent form—do not the innumerable gradations that must have connected them exist also? There is but one answer:—We are ignorant what characters are of essential value to each species; we do not know why white terriers are more subject than darker-coloured ones to the attacks of the fatal distemper; why yellow-fleshed peaches in America suffer more from diseases than the white-fleshed varieties; why white chickens are most liable to the *gapes;* or why silkworms which produce white cocoons are not attacked by fungus so much as those which produce yellow cocoons. Yet in all these cases, and many others, it has been shown that immunity from disease is correlated with some slight difference in colour or structure; but as to the cause of that immunity we are entirely ignorant."

In the following passage the author leads up to that phase of Darwinism of which the political economists have been making use:—"It was this constant struggle between the different tribes that weeded out the weak and indolent, and preserved the strong and enterprising; just as among many of the lower animals the stronger kill off the weaker, and the result is the improvement of the race."

Many persons in these days talk glibly about the "battle of life" and the "survival of the fittest," and the improvement of the race. But the question may arise—Who are the fittest? If we look at the vegetable world we find that the species precious to us for food, medicine, or clothing, and the kinds which delight our senses with the grace of their shapes, the beauty of their colours, or the delicacy of their odours, are precisely *not* the forms which would come

out victorious from the struggle for existence. Were it not for man's constant intervention, the vine and the wheat, the rose and the carnation, would soon succumb to a tribe of weeds as devoid of utility as of beauty. Further, when man takes in hand to improve any race, his first step is to stamp out the struggle for existence, whether between individuals of such race or between the race itself and other species. If we examine the case of a turnip-field we find that the number of plants is strictly proportioned to the extent of the land, and that an unrelenting war is waged against all weeds. Were the struggle for existence permitted to rage unchecked we should certainly find some of the turnips larger than others, whether in virtue of more favourable circumstances or of more intense vitality in the seed. But the yield from the whole plot would be trifling, and no root would reach the dimensions attained where every plant finds a sufficiency of room and of nutriment. Perhaps, in like manner, the men most valuable to the world—the discoverers and inventors—are not those who win the ordinary prizes of life. Perhaps society or its rulers may one day find it necessary to take for a model the farmer's management of his turnip-field.

We can do no more than briefly call attention to the author's view of cyclones. From these terrific manifestations of force down to the eddy of dust which we often see crossing a road or a common, we have, according to Mr. Belt, one and the same phenomenon, differing merely in degree. Hence he recommends a careful study of these small dust-whirlwinds, as calculated to throw light upon the origin of cyclones. His own theory, first advanced in 1857, in a paper read before the Philosophical Institute of Victoria, is that the particles of air next the surface do not always rise immediately they are heated, but that they often remain and form a stratum of rarefied air next the surface, which is in a state of unstable equilibrium. This continues until the heated strata are able, at some point where the ground favours a greater accumulation of heat, to break through the overlying strata of air and force their way upward. An opening once made, the whole of the heated air moves toward it, and is drained off, the heavier layers sinking down and pressing it out. That hot air does not always immediately rise is proved by the hot winds of Australia, which blow from the heated interior towards the cooler south, instead of rising directly upwards.

In conclusion, we feel bound to give this book our most cordial recommendation. Few men possessing any degree

of scientific culture will be able to read it without deriving both pleasure and profit, or will refuse to join us in the wish that this may not be Mr. Belt's last appearance before the public. It is no idle compliment to say that if he were a man of leisure, instead of a man of bustle and business, he would prove a worthy rival of Agassiz and of Humboldt.

V. ANNUAL INTERNATIONAL EXHIBITIONS.

By Fred. Chas. Danvers, Assoc. Inst. C.E., &c.

THE undoubted success which attended the two great International Exhibitions of 1851 and 1862 were unmistakable evidences of the popularity of those institutions, and the public, by the support they accorded to them, testified plainly enough to the correctness of the judgment which allowed a decade to elapse between the two events. Another similar occurrence in 1871 or 1872 would, no doubt, also have proved a success, notwithstanding that intermediate exhibitions elsewhere had, to some extent, anticipated the results which such an institution would have achieved. Nothing could be more advantageous than the establishment of a decennial exhibition of all nations, for the purpose of exhibiting the advances made in science during each period of ten years; but when the example must needs be followed by France, Italy, Germany, Russia, America, and probably other nations also, the Exhibition necessarily becomes one of almost annual occurrence, and the strain upon manufacturers becomes too great for endurance, and it soon manifested itself that such Exhibitions were degenerating into huge advertising affairs, and they were treated in just that light by manufacturers in general. Hence it was clear that the days of huge International Exhibitions were numbered, and that it would be impossible to extend their existence for any further lengthened period.

Whether it was from an appreciation of this fact, or from other causes, that led to their abandonment in this country, we are unable to state with any certainty; but on the 23rd of July, 1869, their death-warrant was signed, so far as this country is concerned, by the issue of an advertisement by Her Majesty's Commissioners for the Exhibition of 1851, wherein the public was informed "that the first of a series of Annual International Exhibitions, of selected works of fine and industrial Art," would be opened in London, at South Kensington, on Monday, the 1st of May, 1871. It

was then contemplated that these Annual Exhibitions should be devoted each to certain classes only of industry and fine art, and it was so arranged that within a cycle of seven years all the principal arts and manufactures of the world would have been illustrated in them.

Before entering farther into an account of those Exhibitions that have hitherto been held, it is right that we should state that, prior to the issue of the advertisement above referred to, the whole question of Annual International Exhibitions was fairly laid before the leading representatives of Trade in London, and their opinions were invited as to the probable success of such Exhibitions. We are informed by one of the gentlemen so consulted that the unanimous verdict of the London Trade was that such Exhibitions not only could not succeed, but that they would be mere superfluities; that similar exhibitions, only on a far more extensive scale than could be achieved at South Kensington in the manner proposed, already existed, and were perennial, in all the leading thoroughfares of London; and that a person wishing to become acquainted with any particular branch of manufacture could do so, far more successfully and speedily, by visiting the show-rooms of a West End tradesman than by an inspection of anything that his branch of trade collectively would be likely to exhibit.

Anyone who has been a studious visitor to the Annual Exhibitions already held will be able to bear out completely the views expressed above. These Exhibitions have clearly not commended themselves to manufacturers generally in this country, whilst foreign nations have certainly been but very imperfectly represented on every occasion, although perhaps some exception may be made as regards the French *Annexe*, but then, as must have been apparent to all, the French Exhibition has not generally been devoted to the special classes of objects for which each year's exhibition was supposed to be devoted. But, independently of this. the French *Annexe* has, in a great measure, been the means of bringing these Annual Exhibitions to an untimely end. By this remark we do not mean to imply that there was anything in the principle or manner of conducting these Exhibitions that rendered their continuance even desirable; on the contrary, we were from the first impressed with a conviction that their ultimate failure was inevitable, but we were, perhaps, hardly prepared for their meeting with so speedy a dissolution. It appears that the French Government was induced to go to the expense of erecting its own *Annexe* at South Kensington, upon the distinct understanding

that the exhibitors therein should be permitted to sell their goods, thus converting that part of the Exhibition into a bazaar or shop, whilst in other parts the exhibitors were often prohibited from even putting a person in charge of their several exhibits. From the reports on the 1871 Exhibition, published in Paris, we learn that in that year the articles sold in the French *Annexe* during the Exhibition were valued at over £20,000. This proceeding on the part of the Commissioners had, of course, the effect of handicapping all other exhibitors besides the French, which raised a storm of indignation, especially amongst the English exhibitors, which the Commissioners were unable to withstand; indeed many manufacturers positively refused to exhibit any further unless the odious distinction were withdrawn, and that either all or none in the Exhibition should be allowed to convert their stalls into shops. In consequence of the firm stand made by the English trade representatives an attempt was made to prohibit the selling license to the French exhibitors, and the consequence of this was that the French Court was not opened at all last year. This year it is again accessible to the public, and purchases may be made as freely as ever; for notwithstanding the prohibition against the removal of goods until after the close of the Exhibition, we are aware of one instance, at least, in which the delivery of goods purchased in the French Court has already been effected.

The inevitable consequence of this course of proceeding on the part of the Commissioners must, under any circumstances, have led to the early collapse of these Annual Exhibitions; but we understand that, besides this, other influences have made themselves felt which have induced the Commissioners to declare that this year's Exhibition shall be the last. The fact is, we believe, that these Exhibitions are not attractive enough to the public, and the necessary consequence is that they do not pay. In other words, these Exhibitions are not popular, and no better proof could be desired of the indubitable fact that they are not only not required, but that they are evidently looked upon as unnecessary *amusements* for the London public.

Looking upon these Exhibitions as Institutions for the Advancement of Knowledge, we think that the small attractiveness which they present to the general public is a sufficient condemnation of them from that point of view. As places of amusement they fail, because the London public is sufficiently educated to know how to discern between what is excellent and what is worthless. As Indus-

trial Exhibitions these annual institutions have lost their hold on the public faith, and they have come to be looked upon more as bazaars than as International Exhibitions in the highest sense. Looking at these Exhibitions from a "South Kensington" point of view,—that is, as media of instruction to the public,—we believe that they might be made to pay, but then an entirely different arrangement to the present must be made for that purpose. Instead of inviting manufacturers to exhibit their goods at their own cost,—which can but end in an imperfect representation after all,—three or four manufactories might be exhibited in their most entire completeness, and visitors should be allowed to purchase and carry away any articles they may have seen manufactured. In order to carry out such a programme the manufacture must be complete in all its details; but for the laying down of the necessary plant and machinery the Commissioners would, of course, have in the first instance to bear all expenses, and perhaps even pay people to set up their factories within the Exhibition; but in all cases the manufactory should be complete, and the Exhibition should not only consist of a display of manufactured articles. With what interest would the blowing of a retort of Bessemer steel be viewed by the fair inhabitants of South Kensington,—at a safe distance from the sparks, and viewed of course through a spectroscope,—and with what delight would the young men of London hasten to see a huge casting run direct from the cupola, or a massive bloom of iron writhing under the thud strokes of a 50-ton steam-hammer? We were fortunate enough to see some of the glass manufactured for the Exhibition of 1851, and can testify to the attraction which such a display would yield to the London public; whilst the manipulation of glass in its finer forms—such as in the manufacture of wine-glasses, decanters, or tumblers, and the subsequent processes of their cutting and ornamentation—would contribute no slight entertainment to the sight-seeing people of London. We are perfectly well aware that these sights are all to be seen within the great city of London itself, but they are not so accessible to the general public as the display of the manufactured articles themselves. The only question for the Commissioners to consider is whether such displays could safely be carried out on the present site. The public have now had over twenty years of exhibitions of manufactured articles, and it appears that the time has now abundantly arrived when the process of manufacture should also be exhibited more fully than has hitherto been attempted.

We refrain from entering into any further details on this subject on the present occasion, but having thrown out the idea in a somewhat vague way, perhaps, we proceed now to review, as briefly as possible, what have been the results of the past four Annual Exhibitions, including, of course, the current one which has not yet closed its doors upon the unappreciating public.

The Exhibition of 1871 was confined mainly to three classes, viz., fine arts, pottery, and woollen and worsted fabrics, together with machinery employed in the manufacture of the two latter classes. Besides these, however, there were also exhibited educational appliances and instruments, scientific inventions and new discoveries, and horticulture. Amongst the machinery employed in the manufacture of pottery there were brick- and tile-making machines, clay-crushers, pug-mills, pottery wheels, pipe-making machines, stone-breaking machines, whilst in another part of the building was exhibited every conceivable variety of finished products of the potter's art, from the exquisitely finished designs of Wedgwood, Minton, and Daniell, to a ginger-beer bottle and tobacco-pipe. Amongst the machinery for woollen and worsted manufacture were exhibited various power looms, combing machines, balling machines, carpet-weaving machines, &c., &c., whilst the manufactured articles were also exhibited in endless variety. It would be out of place, upon the present occasion, to enter into detail regarding what was exhibited so far back as 1871, but it may be generally remarked that, whilst some of the exhibits were decidedly novel, many were of an uncertain age, and the Exhibition generally could not be looked upon as in any way representing the fullest advancement that had been developed in the special classes exhibited. Scientific inventions represented upon that occasion formed the subject of an article in the "Quarterly Journal of Science" in the same year.

The second of the series of annual International Exhibitions was opened on the 1st of May, 1872. As in the former year, this Exhibition consisted of three main divisions, viz., fine arts, manufactures, and recent scientific inventions and new discoveries. The leading manufactures represented on this occasion were cotton, cotton fabrics, jewellery, paper, stationery, and printing, whilst musical instruments of all kinds, and acoustic apparatus, were also represented. Paper formed the principal topic of exhibition, and it was dealt with at some length, at the time, in a special separate article. Notwithstanding the great importance of the

classes selected for exhibition, it cannot be said that they were well represented so far as the mechanical part of the Exhibition was concerned, although there was a fair display of manufactured goods. As regards scientific exhibits more care was displayed by the Commissioners in the selection of objects, so that fewer trivial articles found their way in than was the case in 1871.

Class II. in the Exhibition of 1873 comprised silk and velvet fabrics, steel and cutlery, surgical instruments, carriages not connected with either railways or tramways, substances used as food, and, finally, cooking and its science. One noticeable feature in this Exhibition was a falling off in the machinery department, which may perhaps be partially explained by the existence of a formidable rival at Vienna, where, it may be presumed, most articles worthy of being exhibited were sent, in preference to South Kensington. This year the industrial arts represented at South Kensington include leather, hand- and machine-made lace, civil engineering, architectural and building contrivances, sanitary apparatus and constructions, cement and plaster work, heating by all methods and kinds of fuel, bookbinding, and foreign wines. The mechanical exhibits are—machinery for lace, leather, bookbinding, civil engineering and building, and such as are included under the head of recent Scientific Inventions.

At best, the present Exhibition cannot be said to be in any way an improvement upon its predecessors; in some of the classes it is very poorly represented, and upon the whole it decidedly lacks interest. Indeed, a visit to South Kensington is quite sufficient to prove, to the most casual observer, that these annual exhibitions are gradually dying out from natural causes.

The great International Exhibitions have grown at so rapid a rate that it is very clear that they could not long be continued, upon their original basis, in consequence of the vastness of the buildings required for their reception. But in endeavouring to limit their proportions by spreading them over seven years, their interest has also diminished in proportion, not only to the visitor, but equally so to the exhibitor, and hence the cause of their failure. In a recent letter to the "Times" newspaper (Tuesday, June 9), Dr. Forbes Watson entered in some length upon the great question of Exhibitions, and, while admitting the failure of the present organisation, he recommends that instead of endeavouring to make them popular, class exhibitions should continue to be held, under a revised system of arrangement and manage-

ment, more in the interests of respective trades than of an unsympathising public. With regard to the present system he argues that the "conditions under which they occur, the brevity of their existence, the multiplicity of similar objects, the impossibility of a systematic arrangement, make it all but impossible that any profit should be derived from them except to those already acquainted with the subject. International Exhibitions," he goes on to state, "might be utilised less for the purpose of increasing the number of people possessed of technical education, than for raising the level of knowledge among the classes already possessed of the fullest information regarding each of the subjects represented at the Exhibition." And this information once acquired should, it is further suggested, be rendered permanently available to the country by being embodied in museums.

Theoretically, this suggestion looks very well upon paper, but practically we very much doubt the possibility of its being carried out. The true advancement of manufacturing industry will, we confidently predict, never be demonstrated in such exhibitions, for in most trades there are so many secrets—each manufacturer possessing certain knowledge of his art which is not patented or otherwise divulged to his competitors in trade—that it is impossible to hope that the ultimate stage of advancement and knowledge will ever be represented in public exhibitions. Indeed, it is to these exhibitions chiefly that we owe the vast amount of foreign competition in manufactures, not only abroad, but even in our own markets.

Whatever may be the next stage of International Exhibitions, it is to be hoped that a reasonable time will be permitted to elapse before anything else of the kind is attempted; indeed, it seems necessary that such should be the case, for, the present annual series having come to a premature end, a fresh system and organisation will necessarily be required before anything new can be attempted.

VI. THE IOWA AND ILLINOIS TORNADO OF MAY 22, 1873.

By JAMES MACKINTOSH, M.A.

THIS tornado could not—by its destructive effects upon trees, buildings, &c.—be traced further back than Section 35, Warren Township, Keokuk Co., Iowa. At the point where the destruction commenced, the South Skunk river bends southward, and then again to the east, thus half inclosing a low level area of about 1 mile in length by ½ a mile in breadth. This bottom land is surrounded by bluffs about 70 feet in height on nearly all sides, the river flowing close to the bluff on the S.W. It was at the north-east edge of this natural amphitheatre that the tornado first attained force sufficient to demolish fences. It is difficult and unnecessary to determine its starting-point: there is, however, no doubt of the fact that it first attained to desolating violence on the farm of W. W. Morrow, situated half-way up the bluff. Its previous history was only that of a thunderstorm, accompanied, perhaps, by an unusual tumult and whirling of the clouds. In tracing its development and progress, therefore, I probably labour under no disadvantages which do not necessarily attach to the history of all such meteors, except those arising from the circumstance that the tornado was not at any point of its progress witnessed by a skilful meteorologist.

Mr. Morrow testified that neither he nor any of his people noticed any funnel-shaped appearance or tongue of cloud approaching the earth. There was a strong wind for a minute, but the destructive gust appeared to be instantaneous. A smart shower of hail followed the gust. A whirling of the clouds was observed several minutes before the wind. A little lightning was seen. The storm travelled E.N.E. over Andrew Surber's farm, blowing down fences with a S.W. wind. He saw the clouds whirling like a great wheel, 35 degrees in width, before the storm, but did not notice any funnel appearance. Some hail fell with the wind, and a considerable shower of rain after it.

John Malcum, Section 30, Lancaster Township, was 1 mile south of tornado when it occurred. Heard a roaring like steady thunder in the west about a quarter of an hour before the storm came. Saw no lightning. A cloud covered the western, the northern, and north-eastern portion of the sky. It extended somewhat beyond the zenith. The rest

of the sky was clear. Saw a mass of clouds whirling contrary to the hands of a watch. The whirling mass appeared to be about 60 degrees high when directly opposite. A smart shower fell some time after the tornado.

On Malcum's farm the roof of a stable was blown to the south-east. This was the first damage done by a north-west wind. Two hundred yards south of the stable a fence was blown toward the north-west and north. The storm, after becoming destructive, had travelled $1\frac{1}{2}$ miles before it developed force sufficient to commit destruction by a north-west and south-east wind.

The breadth of fences thrown down on the farm of T. Dawson, Section 31, Lancaster Township, was about 60 rods. The outhouses, &c., were damaged. A cultivator weighing about 200 lbs. was carried or dragged 30 feet; it presented to the wind a surface of not more than 3 square feet. If it was carried, the lifting force of the wind must have been between 60 and 70 lbs. per square foot. The ground showed no signs of its having been dragged.

M. Williams, lawyer, Section 32, Lancaster Township. watched the storm as it approached for about one hour; it was a few minutes after 2 P.M. A cloud rose in the west, which, stretching to the north-west, presented the appearance of heavy rain. Previous to the approach of this cloud the sky was nearly clear. The wind during the day was southerly. About twenty-five minutes after the tornado the wind was again from the south-west. The storm-cloud did not extend far past the zenith to the south. Saw the funnel distinctly. It alternately lengthened and contracted, rose and fell. When it contracted, it appeared as if the narrow point next the earth was cut off, leaving the lower end broader. At times the upper end appeared to reach the overhanging clouds, and at times to be not so high. It was of a dark blue colour when 250 yards distant; when 300 yards distant it subtended an angle of about 75°; the angle subtended by the top of it at that distance was about 55°. It had a zigzag motion. Half an hour previous to the tornado there was incessant lightning in the north-west. Heard no thunder. There was no lightning in the tornado. A little rain and hail fell just before the tornado, and a smart rain-shower about twenty minutes after it. Heard that to the northward there was a terrific storm of rain and hail, accompanied with thunder and lightning. The wind was south generally during the day. As the tornado approached the wind changed to a little east of south. Saw the dark funnel strike the ground on my farm. Saw it whirling contrary to the hands of a watch. This witness

was stationed about 25 yards south of the storm-centre when nearest to it. The fences were generally thrown toward the centre on either side, but where the dark cloud touched they were carried away for a space 60 yards wide. We have here the first evidence of the dark cloud touching the earth in perfect funnel-form; but its touch is yet only temporary: it proceeds with a wavy, zigzag, circular-pendulum motion. By-and-bye its tread will make the earth tremble.

The storm traversed Jones's farm, throwing down fences, until it struck the Wolfden school-house, which lay near its centre. The school was in session when the tornado struck it.

Richard Weller, teacher, testified that this occurred at 2.15 P.M. precisely. This time is valuable, and I have adopted it as one of the data for calculating the velocity of the storm. The school-house was moved, with the children and teacher in it, to the east, the north end 30 feet, the south end 20 feet. It was not overturned. The windows, roof, &c., were much damaged, but there were no evidences of explosive forces. The weight of the building was given by Mr. Williams as probably 30,000 lbs. The surface exposed was 360 square feet, besides the slanting roof. The slant of the roof was about 45°. The foundation was stone. It became very dark as the tornado struck. After leaving the school-house, which is situated in a slight ravine-like depression, the fury of the tornado abated somewhat: hence, although it was nearly central over the hamlet of Hayesville, the frail houses were scarcely touched.

The storm up to this point had been travelling E.N.E. Since leaving South Skunk river it had been traversing a rolling prairie, with numerous sloughs, as they are called, but nothing like a water-course. It struck Troublesome Creek, the banks of which are well wooded. No sooner did it do so than it increased greatly in power, changed its path temporarily to due east, and developed the phenomena of two or more funnels or branches of a funnel. Down in the hollow, among the trees, stood the house of widow Jacobs. It was completely demolished, but without signs of explosion. The storm-traces are already in great part obliterated, and a new house rebuilt. The path of destruction was 200 yards wide at this point, and the general aspect of the fallen trees within this limit presented all the appearances of a complete cyclone revolving contrary to the hands of a watch, although nothing particularly worthy of notice presented itself. About a quarter of a mile from the main

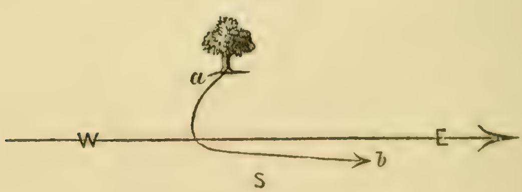
track to the south, a swath, about 50 yards wide, was cut through the fences by a south-west wind. This swath appeared to curve towards the main storm-path, but it was not possible to follow it until it reached it, because of the sparseness of the fences, and because there had everywhere on that side of the storm been a strong south-west wind, and this swath was merely exceptionally strong.

The storm-centre next traversed the Grout Farm, now occupied by Samuel Brunt, and passed about 100 yards to the north of the village of Lancaster. Here its operations became more interesting.

Samuel Brunt heard it roaring a long time before it arrived; as it approached saw two funnels distinctly: their summits were lost in the overhanging mass of dark cloud. Saw funnel on the south side, which was the smaller, swing around in a half-circle and join the larger one. The funnel had a pendulum motion. When it struck the ground it seemed to smoke, the smoke surging up like spray upon a wave-beaten rock. The wind felt cold as it passed. Saw lightning during tornado. The breadth of the dark apex of the main tornado, where it touched the ground, appeared to be about 100 feet: there the wheat was mown as with a scythe.

The breadth of the part of the storm of sufficient power to throw down fences was here 200 yards. The fences on the north side were blown south, those on the south side north, while along the centre everything was carried east with the storm. An apple tree, 7 inches in diameter, which had stood on the north side of the centre, was carried first 14 yards south, with a little westing, then round in a regular curve 24 yards toward due east, and then due east for 70 yards. A beam, 2 ins. × 4 ins. × 14 feet., and weighing 25 lbs., was driven into the ground 3 feet 9 inches at an angle of 35°, after having been carried 35 yards from the barn roof. The following is a sketch of the path of the apple tree: it stood at *a*, and was carried to *b*.

Fig. 8.



Fred. Tollman, Lancaster Village, was within the edge of the tornado. Was whirled like a top for 50 yards, and lodged against a fence. Observed it for more than fifteen minutes before it came. Saw trees twisting in the grove half a mile distant. Saw the funnel whirling contrary to the sun ; its summit was lost in overhanging cloud. Thinks the upper cloud was also whirling. Saw only one funnel. Saw lightning flash up and down the funnel. Saw a tree thrown out from the top of the funnel about 1 foot in diameter. Top of funnel 60 degrees high when 70 yards off. There were two funnels with the small ends together, thus, the upper one being the largest :—

FIG. 9.

The evidence for the village of Lancaster is somewhat discordant, or rather various. Some saw only one funnel; some two funnels, side by side ; and others two funnels superimposed, with the small ends together. The discrepancies are, however, easily accounted for. As the cloud approached it grew very dark. The tornado doubtless changed its form rapidly. The observers took a glimpse at it, and ran to attend to their houses, or had to watch it under difficulties. Those who saw two funnels appear to have seen through the centre of what appeared to those at a greater distance as only one funnel with the larger end down. The evidence afforded by the ruins in the path of the storm gives no support whatever to the belief that there were two distinct whirlwinds. The two dark clouds touching the ground worked together in the strictest harmony in producing such effects as would be produced by one tornado

whirling contrary to the hands of a watch. But, as already instanced, streaks of wind of unusual power curved in half-circles toward the centre at certain places. One of those streaks passed in a north-easterly direction through the town of Lancaster, while the main whirl was 100 yards to the north of it. This streak was at first only a few yards broad, but rapidly increased in width as it proceeded. It first unroofed a frail stable, without injuring the house beside it. It then increased in force, throwing down four or five houses, and unroofing as many more. The following shows the position of the houses and the directions in which they were blown:—

FIG. 10.

The long arrow denotes the main track; the curved arrow the streak mentioned; the short arrows show the direction of the destructive wind which threw down the various buildings. Between the building *a* and the main storm-path there is a space of 50 yards comparatively uninjured. The sketch does not pretend to be strictly accurate as to distances, but to give the relative positions of the houses.

Leaving Lancaster, the whirlwind travelled down the declivity toward North Skunk river in a direction somewhat east of north-east. It levelled the fences in its path, in the way already described, and which never varied from its commencement to its end. The house of widow Dogget, situated about a quarter of a mile south of the river, stands a little to the north-west of the storm-centre, at the commencement of the level bottom lands. Here two deep ravines—one from the south and one from the east—meet. Here the storm developed enormous power, smashing up the timber terribly. The roofs of the house and barn were carried south and somewhat west. The trees on the north side of the centre were thrown down toward the centre, some pointing south-west and south, but the majority south-east. On the south side of the centre they lay pointing north-east, north, and north-west. An oak tree, 12 feet in circumference at the base, was broken across 12 feet from the ground. At the bottom of the narrow, steep ravine,

running east and west, trees were lying just as they fell, some pointing east and some west.

One quarter of a mile beyond Mrs. Dogget's house the tornado reached North Skunk river, which it then followed for over 2 miles, in a south-easterly direction, until it came to the southerly bend of the river, opposite Kohlhaus's saw-mill, when it suddenly turned to the north-east. The bottom lands on both sides of the river are here covered with tall timber, and through this timber the storm tore its way with resistless fury. All trees within a breadth of about 100 yards were either overturned or broken off and barked. The barking was evidently the result of three things:—

1. Many of the trees barked had been bent, so as to loosen the fibres of the wood. Such bending would infallibly loosen, and perhaps break, the bark.
2. The air was thick with missiles, which not only broke the bark, but sometimes penetrated the wood. I have seen a corn stump less than 2 inches in length sticking in the bark of a large tree which it had penetrated.
3. The peeling off of such loosened and broken bark would be a light task for such a tremendous blast. There was not the slightest evidence of electric action or of vapour explosion.

Broken trees had the bark torn off, both up and down, from the point of breakage. Trees half broken and bent to the ground were similarly stripped.

At the place of rupture the trees generally presented that broom appearance in which the supporters of the theory that electricity is the principal agent in the production of tornadoes have seen conclusive evidence of its truth. It will afterwards be shown, when this phenomenon becomes more common than could be expected on the soft soil bordering North Skunk river, where trees were more easily uprooted than broken, that this broom appearance is due to the excessive bending and straining of the fibres of the wood, and their rupture, in succession, from the exterior inwards.

The river, being impassable, rendered the examination of the general position of the fallen trees more difficult. Everything, however, corroborated the evidence already so abundantly adduced, that the tornado was a whirlwind of powerful centripetal tendency, circling contrary to the hands of a watch. The following arrangement of fallen trees was selected as being typical. It was found on the north side of the river, near the centre of the tornado.

No. 1 is a tree 3 feet in diameter, but much decayed, blown down from the E.S.E. No. 2 is a tree $\frac{1}{2}$ foot in diameter, blown down S.S.E. No. 3 is a tree 2 feet in diameter, blown down from the W. No. 3 lies above No. 2, and No. 2 above No. 1. It occurred just where the storm left the river.

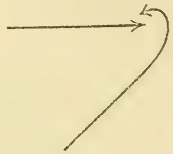
FIG. 11.

That the force of the storm was enormously increased immediately after reaching North Skunk river is proved by the greatly extended width of its path. At A. Dogget's and B. C. Moore's houses, three-quarters of a mile distant from the storm-centre, the out-buildings were damaged and the fences thrown down by a southerly wind. Mr. Dogget testified that the tornado appeared somewhat like two funnels with the smaller ends together, and that when apparently $3\frac{1}{2}$ miles distant the column subtended an angle of about 25°. The houses of these gentlemen are situated on the summit of the bluffs facing the river, which are perhaps 200 feet high.

The house of Joseph Ash, situated on the face of the bluff, and distant from the river about half a mile, was blown to the S.S.W. Large trees were at this distance blown down in the ravines running to the river. Mr. Ash testified that the wind lasted a few minutes, and that it changed to the north-west after the passage of the tornado. A little hail fell before the storm, and a small shower of rain after it. Saw lightning and heard thunder. It grew cool as the storm passed.

The following represents the apparent direction of motion of the two funnels as they approached each other :—

FIG. 12.



The house and steam saw-mill of Joseph Kohlhaus stood directly in the path of the meteor, at the distance of a quarter of a mile from the river, on the north side.

The buildings stood upon the summit of a rising ground, about 30 feet high. Between it and the river is a half pool, half marsh. It was completely emptied of water. The breadth of the path of wholesale destruction was, upon the summit of the hill, 270 yards. The house stood 100 yards from the eastern and 170 yards from the western extremity of this path. The timbers and contents of this house were carried in a half-circle first to the north-west, then to the west, then south-west, then south, and finally to the north-east, the heavier articles being generally sifted out first, thus marking the way the ruins went.

On the eastern edge of the path was an orchard, and on the western a wood. The trees in the orchard were blown down from the south-east and south; the trees in the wood from the north and north-west. Portions of the clothing from the house and shingles from the roof lay among the trees in the wood, or stuck among the branches. There were no signs of explosion, the doors and windows having been blown in. The inmates were all more or less injured. The width over which fences were blown down was here about three-quarters of a mile. An iron plough, weighing 200 lbs., was carried 40 yards. The sheet-iron chimney was carried 2 miles to the north-east. An iron sausage-machine, 6 inches by 8, and weighing 15 lbs., was blown away; part of it was found 1200 yards distant. The wheels of waggons were smashed, and the tyres twisted.

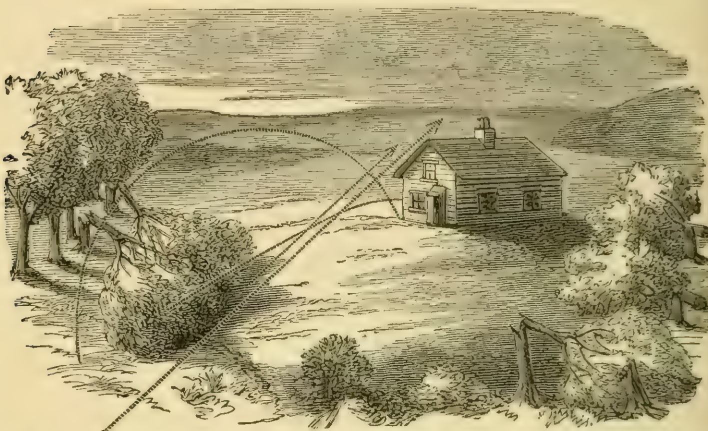
Fig. 13 is a representation of the effects of the whirlwind at this place.

After leaving the mill the storm ascended a hill some 200 feet in height, levelling the fences, but with evidently diminished violence.

Passing over the top of the hill in a north-east direction, it struck Rock Creek, which here runs south, with a little

easting. Immediately altering its course, it went straight up the creek for about half a mile, developing prodigious power. The large trees which lined the banks were torn and peeled and overthrown in promiscuous ruin. At a point

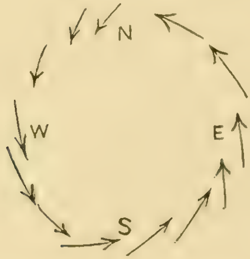
FIG. 13.



where there is a small circular island, and a considerable amount of stagnant water around, the storm would appear to have stood still for a moment. Fig. 14. shows the position of the fallen trees at this point.

There were no trees on the island itself. There was no evidence whatsoever that the barking of the trees had been effected by electrical action or sudden evaporation. Everything tended to prove that the bark had been loosened and

FIG. 14.



broken by the excessive bending of the trees and by flying missiles. One of the steers killed at this creek had an oak rail driven completely through its shoulders.

On reaching a slough which enters Rock Creek from the north-east the tornado followed it. Beside this slough and close to the creek was a small grove of young trees belonging to John Stein. This grove was completely carried away, nothing remaining except a few barkless twigs. At no point in its course did the storm develop greater energy than at this grove. Generally young saplings, over which the centre of the storm passed, were, although stripped of bark and twigs, left standing. Here the ground was whipped as bare as though the grove had been lashed with a whirlwind of fire. The tornado then passed between the houses of William Goeldern and John Stein, sweeping the fields, within the narrow path of its greatest violence, clean of grass, wheat, and corn stumps, while the ground was torn and furrowed by flying rails and trees. The rails and broken timber had been gathered from the fields; but John Stein assured me that they lay thickest along the centre of the storm. Along the sides of the path of greatest violence many rails were driven deeply into the soil, end foremost.

While the main whirlwind thus pursued the path described, there were smaller off-shoots or arms which played havoc on its south-east side. Such an arm cut a swath about 12 yards through Schild's orchard from south-west to north-east, the swath widening as it went. It was not possible to trace it until it reached the main storm, for reasons similar to those already given. It seems probable, however, that it joined it at the little island above mentioned, where the storm appears to have momentarily stood still. The relative positions favour such a surmise. This streak of strong south-west wind was a quarter of a mile distant from the centre of the main tornado.

A similar streak levelled the fences close to the house of John Stein for a width of 50 yards, while nearer the main tornado the fences remained standing. This streak, I surmise, joined the main storm at the grove above mentioned. A similar arm tore down the new barn of F. A. Latz, about a quarter of a mile farther on, without injuring weaker buildings close beside it. The path of this streak is said to have been very distinct among the fences until it joined the whirlwind near the house of Peter Marsh. The fences were, however, already restored.

George Star, three quarters of a mile south-east of the tornado, said that about 1 P.M., it commenced to thunder and lightning in the north-west. The storm advanced along the northern sky, and it lightened terribly there previously to the arrival of the tornado. A big cloud extended from

south-west to north-east. It cleared up immediately after the tornado, both here and in the north. This witness lost eighteen head of cattle at Rock Creek. One was perforated by a rail.

John Marsh's house stood a little to the north-west of the centre of the storm. He watched it as it came directly toward him. It deflected a little from side to side, with a zig-zag motion. It turned and twisted like a screw in revolution. All the family were in the house, the house having no cellar. It became as dark as midnight. The windows and doors were blown in, in spite of resistance, and they knew no more until they found themselves lying in the slough, severely wounded.

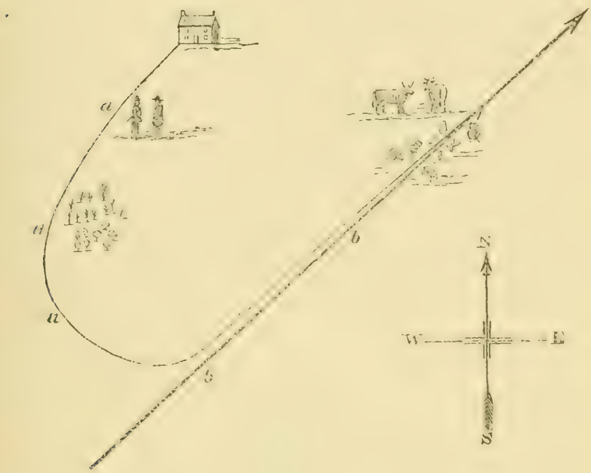
The destruction at this house was most complete. The house was carried forward from its foundations bodily; but as it was going down a declivity toward a slough, it failed to strike the ground until it went to pieces. The heaviest timbers and the inhabitants were deposited in a slough, about 100 yards S.S.W. of where the house stood. One child was killed instantly, and Mrs. Marsh has since died. The fragments of the house were carried, first to the south-west, then in a curve to the south, the south-east, the east, and then along the centre of the storm to the north-east, the heavier articles sifting out as they went. I found ears of corn 200 yards to the south-west and south of the house, and a fragment of heavy wood 400 yards to S.S.W. Two cows and thirteen hogs were in a yard a little nearer the centre of the storm than the house. They were carried 100 yards to the south-east, and killed or fatally injured. A stone from the foundations of the house, weighing about 100 lbs., was said to have been carried about 100 yards south; but I could not find it. Sowing machine, cultivators, wagons, &c., were wholly carried away, and left not a wrack behind. The timber was generally reduced to the dimensions of fine fire-wood, and thickly strewn along the path of the storm to the north-east. The width over which fences were thrown down was here about half a mile.

Fig. 15 is a sketch of the path of the ruins of Marsh's house, *a a a* representing track of ruins of house, *b b* the centre of path.

A quarter of a mile to the north of Marsh's house stood the house, barn, &c., of M. Fuh. They were blown to the south-west, but without being carried away. The next house struck was that of M. E. Hamis, the storm-centre passing about 20 yards to the north-west of it. The fiercest of the storm was here only 100 yards wide, but fences were blown

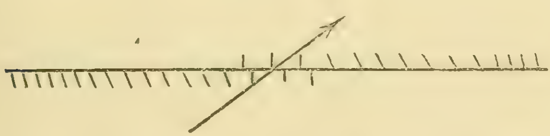
down for over 500 yards. The doors and windows of the house were driven in by the wind and the flying missiles, and immediately the house went to pieces. The house and out-houses were carried away, broken to small pieces, and deposited for a mile or two along the path of the storm. The trees around the house, which were not plucked out by the roots, had their tops and branches broken off and their bark stripped from them. The stripping of the bark was

Fig. 15



evidently due to the causes already assigned. The stripping was most complete on the south-west side of the trees. A fence 400 yards in length, and running east and west, was deprived of its boards, the posts still remaining in the ground. Where the storm-centre crossed it, the direction toward which the posts were leaning was intermediate. On the west of the centre the posts were leaning south, with a little easting, and on the east of the storm-centre they were leaning north, with a little westing.

Fig. 16.



The above is something like the disposition of the posts along the line of the fence. The length of the fence thrown

down east of the centre was about one-quarter greater than that thrown down on the west of it.

The corn-stalks in a contiguous corn-field were thus disposed :—Those on the south-east side of the centre curved around from pointing nearly due north to nearly due west. Those on the north-west side curved around from pointing nearly due south to nearly due east. Along the centre all pointed with the storm. This shows the direction of the last wind, strong enough to alter the position of the stalks. (See Fig. 17).

Matthias Linen, a quarter of a mile north-west of the edge of the storm, testified that it presented the appearance of a great column, reaching from the ground to the clouds, and whirling contrary to the sun. It seemed to remain almost still at some places, and then would dart forward. Hail 4 inches in diameter, very irregular in form, fell as the storm passed.

FIG. 17.

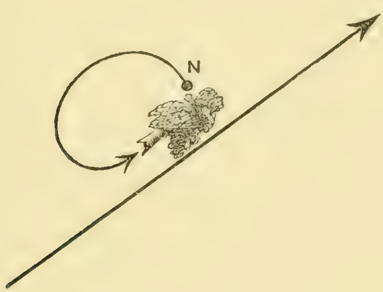
Paul Piffer, Clear Creek Township, Section 9, testified that the tornado looked like a big tree, only it was five times greater at the top than the bottom. It turned like a wheel in a mill. Its direction of evolution was against the hands of a watch. At the distance of a mile, its top, when it entered the cloud, made an angle of about 60°. Hail as large as pigeon's eggs fell before the storm, and it rained very hard after it. Saw lightning in the west previously.

Mr. Piffer's house stands about 70 yards to the north-west of the storm-centre. Its path is here about 200 yards wide. The arrangement of stalks in a corn-field was the same as that already given. The roof of the house was carried south. A stump-cutter, which was standing by the house, was carried—the iron portion 50 yards south, the wooden portion half a mile to E.S.E. Wheels which came

within the reach of the storm were dashed to pieces, and the tyres twisted into all sorts of shapes. An oak tree, 3 inches in diameter, and which stood exactly in the storm-centre, was split by a fragment of a board 1 inch thick. The board was originally probably 6 inches broad and 8 feet long. Of the two small fragments remaining in the tree, the longer was only 17 inches in length. The board was driven into the tree from the south-west. The path of extreme violence, about fifty yards wide, was strongly marked in the grove of young oaks. They looked as if they had first been lashed against a pile of stones and then trailed in the mud. Portions of bark were struck off and the smaller branches shattered and peeled. This grove stands upon the edge of a steep declivity about 100 feet in height, at the bottom of which flows Clear Creek. Here is a circular hollow nearly enclosed by high bluffs, and within it the storm raged with demoniac fury, smashing trees of 4 feet in diameter to pieces.

The following instance was found on the opposite declivity, up which the storm raged with undiminished power:—

FIG. 18.



The tree stood at N originally. Its root was partly decayed, and it must have fallen with the first strong gust. The path which it travelled was cut out in the grass by its root. This path was 47 yards in length, and was nearly circular. It first moved to the north-west and then round in a curve, until it lay pointing to the north-east, with its top almost touching the spot where its root had formerly been. The tree stood to the north-west of the storm-centre, and was 2 feet in diameter.

Crossing the summit of the rising ground, which was thoroughly ploughed up, the storm traversed a wooded ravine,

where for a width of fifty yards it deprived the young trees of their twigs and of much of their bark, besides throwing down the full grown.

The house of Nick Engledinger, Section 10, Clear Creek Township, stood about 100 yards from the south-east edge of the tornado. From any indications which the remains presented, the timbers must have first been carried north. Two persons were killed—one of them being torn to pieces.

A hog weighing 400 lbs. was carried one mile and a quarter in the line of the storm.

The house of Jacob Kœrth stood fifty yards within the north-west edge of the storm. The timbers went south. The people tried to prevent the wind from blowing in the doors and windows, and could not. The wind entered and blew the house asunder, leaving the floor in its proper position. Hogs weighing 300 lbs. were carried to the north-east across a ravine and deposited 300 yards away. A horse, a cow, and a bull were similarly carried 200 yards. Sheep were carried 400 yards. In addition to the accidents inseparable from such an aërial voyage, the bodies of the animals were driven full of pieces of wood. On this account the bodies of all animals which perished in the storm were burned.

A horse-power machine, partly wood, but chiefly iron, and weighing 2400 lbs., was pushed or carried six yards. It was resting on the ground, and was separate from all other objects. There were no marks upon it arising from violent collision with other bodies. It exposed a surface of about 2 square feet to the wind. It lay close to the ground. The master of this house was absent, and information, owing to the invalid condition of the inmates, hard to obtain.

The storm now turned somewhat more to the northward, traversed several fields, demolishing the fences, until it struck the house of R. F. Campbell, Lafayette Township, on the borders of Clear Creek Township. The exact position of this and the two immediately preceding houses was difficult to determine upon the map, because of its very defective condition, and because their owners could not tell me what section they were in.

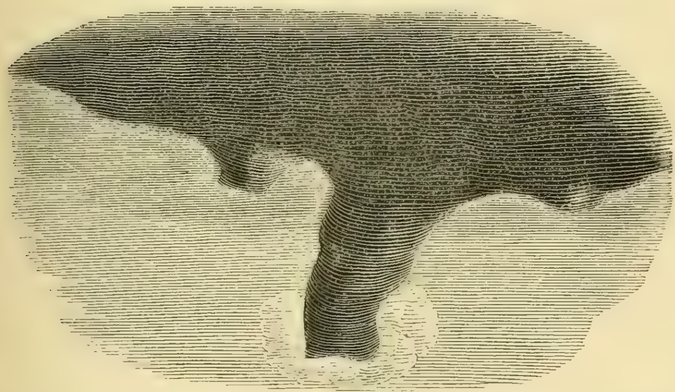
The following is the appearance the vortex presented at the distance of 70 yards. There appears to be two incipient funnels, one on either side.

A post 8 feet in length, and driven into the soil $3\frac{1}{2}$ feet, and 4 inches in diameter, was pulled out by the wind. It stood without attachment.

The width of the storm here is 160 yards. The house

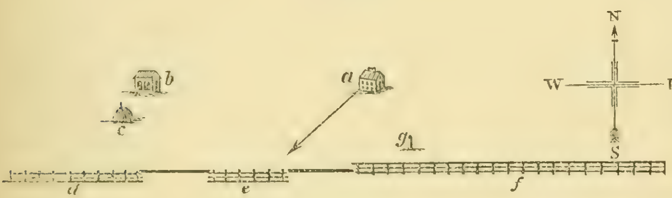
stood within 20 yards of the north-west edge. It was tilted up on its corner bodily, inmates, floor, and all, and deposited on its roof 17 yards to the south-west of its former

FIG. 19.



position, and then went to pieces. There was no indication of explosion. The accompanying is a plan of the effects of the storm at this house :—

FIG. 20.



a is the house which was carried in the direction of the arrow. *b* is a stable and *c* a haystack, both untouched. These were twenty yards from the house. *d*, *e*, and *f* are portions of a fence which were thrown down. *d* and *e* were thrown south, leaving the intervening portions untouched. *f* was thrown north. *g* is the post already mentioned.

This was the last destruction in Keokuk county. The tornado ceased to touch the earth on Mr. Campbell's farm. It drew itself up into the cloud from which it had come down. Hitherto it had traversed a country full of deep and well wooded ravines. Here it entered upon a flat, bare country—in fact, a water-shed. It continues much the same for the seven miles which the tornado here skipped.

The storm having ceased to act as a tornado upon the surface of the earth, I proceeded to Westchester, Washington county, where it was reported to have struck. There can be no doubt, however, that a strong—though not a destructive—wind was felt over the intervening space, and that it obeyed the usual law of cyclones by blowing in spirals toward the point over which clouds continued to whirl like a great wheel.

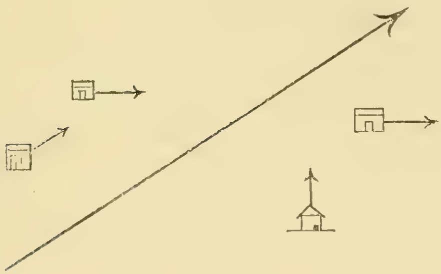
Rev. J. P. Coffman, Cedar Township, Section 33, gave the following evidence:—The tornado arrived about 3 P.M. Heard the noise for more than one hour previously. When it was hailing, heard the sound just as distinct as before. Did not hear roaring after the storm passed. The wind before the storm was nearly due south. After the storm, and with the rain, the wind came from the north and north-west. Have learned that 2 or 3 miles to the north the rain was tremendous. There was lightning in the north-west previous to the tornado. As it approached, saw light fog-like clouds, rushing, with the greatest rapidity, from the north. Its form was not so distinct before as after it passed, when it presented a decided funnel appearance. It was nearly clear in the south. When the funnel was distant about 1 mile it appeared to subtend an angle of about 25°. The funnel might have entered the dark, overhanging clouds, but it appeared to be wholly in view. There seemed at one time as if there had been a violent explosion in the revolving mass, as it was somewhat broken up. Hail larger than pigeon's eggs fell with the south wind before the storm. After the tornado the wind came from the north-west, with rain.

The tornado passed over Mr. Coffmann's house, blowing down fences about a quarter of a mile in width, and damaging buildings. It was all done by a south-west wind.

The storm-centre passed right over the house of John Maughlin, Cedar Township, Section 25. He testified that he saw a complete funnel form about 200 yards distant, and that it went up into an overhanging mass of cloud. The funnel was perfectly opaque, and left a fog behind it, so that nothing could be seen for several moments after it passed. At the distance of 200 yards it appeared to be only 15 degrees in height. Saw no lightning.

The outhouses on this farm were badly damaged. Fig. 21 shows the position of the ruins. The arrows point in the direction toward which the buildings were blown down. The fences were blown toward the storm-centre; those at the centre were carried away.

Fig. 21.



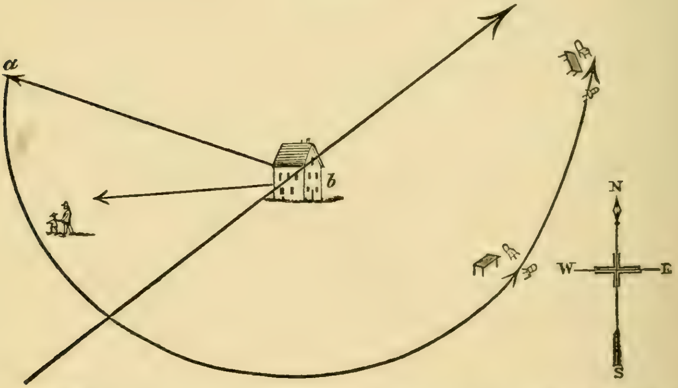
Andrew McKee, Cedar Township, Section 30, said—The clock, which had just been cleaned and regulated, was thrown down and stopped at 3.10 P.M. Previous to the tornado the lightning was warping incessantly like a snake among the dark blue clouds in the north-west. Chunks of ice, about 1 inch in diameter, fell previously to the tornado, accompanied by a light east wind.

I myself saw the clock above mentioned. It still pointed to 3.10 P.M. Mr. McKee's house stands 50 yards within the north-west limit of the storm-path. The buildings were partly pushed, partly blown down toward the S.S.E. The fences were blown to the south-east. A little empty house, about 40 yards to the east, was blown to the south-east, and some of it was carried clear across the path of the storm.

The hedges along the east and west roads are here filled full of *débris*, such as corn stumps, &c. On the south-east of the storm-centre the *débris* is driven into the hedge from the south, and on the north-west side from the north, but there is a greater extent of hedge with *débris* driven into it from the south than from the north. The position of the *débris* driven in from the north seems frequently to have been subsequently affected by a west wind. The hedges running north and south have *débris* driven into them on the north of the centre from the east and on the south of the centre from the west, but the extent of hedge with *débris* driven into it from the west is greater than that with *débris* driven into it from the east. The hedges over which the storm passed looked as if they had been whipped violently against a wall and then trailed in the mud; they were smashed and partially barked, and sometimes carried away. This description applies to the hedges over which the whirlwind, at any part of its course, passed.

George Gilchrist, Cedar Township, Section 29 (house directly in the centre of the storm-track), testified that when the storm struck the house Mrs. Gilchrist, a boy, and a child were in front of the west door, trying to reach the cave, the boy and child being a step or two in advance. The boy and child were instantly carried several rods to the west, while Mrs. Gilchrist was thrust back into the house. The house immediately went bodily, going a little to the north of the boy, who saw it go past like the railroad, as he expressed it. Its sills struck the ground 40 yards from their former position, and the house tumbled over and went to pieces, leaving the inmates comparatively unhurt. As it went to pieces it was struck by a west and south-west wind, the furniture being carried far to the east and north-east. It grew dark as midnight when the tornado struck.

FIG. 22.



Sketch of Effects of Storm at Gilchrist's House. *a* shows the point at which the house was struck, and *b* the position at which the house stood.

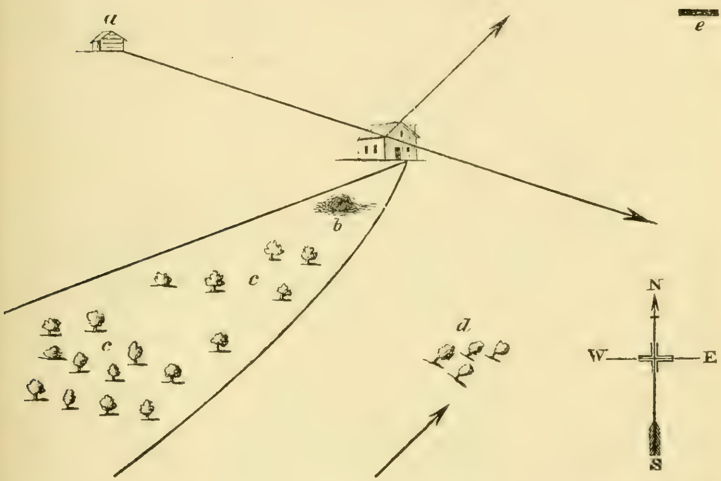
When the storm struck the house of William Caldwell, Cedar Township, Section 29, the inmates were in the kitchen, which was a separate building. The wind blew so hard as to threaten to blow in the south door. Four strong men placed themselves against it: the hinges and lock were partly broken. These four men barely held the door with their utmost strength until the house went in a body: the men found themselves, together with the other inmates, a few yards to the north, lying among *débris;* the door was found a quarter of a mile away. The door presented to the wind an area of 18 square feet.

Mr. Caldwell's house stood on the south-east edge of the most violent vortex. It was first pushed 6 feet to the north-

west, ploughing up the ground. The resistance of the ground and of tree roots here stopped it. It then turned up on its edge, and was then lifted over the tops of trees 20 feet high, without injuring them, and carried 100 feet to the north-east, falling entire, and going to pieces as it fell. The house stood with its end to the south; the area of this end was 280 square feet, or thereabout; the area of the floor of the house was 480 square feet, or thereabout; the weight of the house could not have been less than 20 tons. The trees over which the house was carried are young, and were probably so bent as to allow the house to rise at an angle of 45°.

A heifer, weighing 700 lbs., was carried away by the wind, and thrust head foremost into wet soil until her fore-quarters were buried.

FIG. 23.



Sketch of Mr. Waters's House. *a*, the Granary; *b*, the Cave; *c*, *c*, Trees uninjured; *d*, Trees hurled; *e*, Sill.

Thomas Waters, Jackson Township, Section 19, witness:—Saw the tornado coming rolling on the ground like a wave. Went to the cave, and called upon Mrs. Waters to follow; she would not. Stood in the mouth of the cave, which was 8 yards south from the house, and facing it, and saw the house blown away: it was struck from the south-west. First, a portion of the roof was blown off; then the house went bodily like lightning. When the house had gone I came out of the cave, and was blown to the E.S.E.

Mr. Waters's house was 30 × 16 × 11 feet without the roof. It was carried, sill and all, 24 yards down a declivity without being turned round or tilted over. When it struck the ground, it at first merely shaved it with the foremost sill, gradually going deeper until the resistance became so great as to cause the house to turn over, when it went to pieces. The ground was ploughed up about 2 feet at the deepest point. The house had fallen 3 feet in travelling 24 yards, Its weight was at least 10 tons.

Between the cave—which was only about 4 feet above the surface—and the house, but nearer the cave, stood a small tree; against it a spade was leaning. Mr. Waters testified that it was not blown down. Two yards east of the tree stood a bucket containing a small quantity of lime, and weighing 10 lbs.: it was not disturbed. To the south-west of the cave the trees (*c*, *c*) were comparatively uninjured over a triangular space, while on either side they exhibited signs of the greatest violence. An oak sill (*e*) which had evidently been carried with the house until it struck, and then hurled due east, was driven 4 feet into the soil, at an angle of 45°. Its dimensions are 16 ft. × 8 ins., and its estimated weight 300 lbs. It was found 18 yards east of where the house struck.

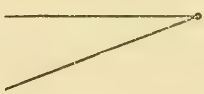
Mrs. Waters was carried with the house until it went to pieces, and received injuries of which she died. The fragments of the house were carried far in the line of the storm.

Alexander Gibson had two houses, both situated as nearly as possible in the centre of the storm. The first house struck was taken away without touching the ground, and went to pieces in the air. The other house, in which were seven persons, was swung round on its north-west corner, as on a pivot, the south end travelling 36 feet, and cutting up the ground as it went. It then turned over, and was demolished. During this journey the floor was broken up, and three persons fell into the cellar. The house was a large one. It afforded no good basis for calculation, on account of the irregularity of its shape. A granary was pushed due east 14 feet: its dimensions are 16 × 8 feet; it is strongly built of generally hard wood, and divided into compartments: it was full of grain. The weight of the building and grain was given at 60,000 lbs. The resistance to its forward motion was very great, because it was surrounded with wet straw and rubbish, of which it had pushed quite a pile before it: it was racked by the strain which had been put upon it. Missiles had been driven through three hardwood planks, in succession, of an inch thick; they consisted

of small pieces of wood, but had been removed. A plank 16 feet long, 2 inches thick, and 1 foot wide, was driven 4 feet into the soil, at an angle of 45°. A corn-sheller, weighing 640 lbs., was carried 400 yards, and destroyed. Trees were barked, but without any symptoms of electric action or of explosion. The general position of trees and ruins was entirely corroborative of what has been already proved by an overwhelming mass of evidence, viz., the rotation and direction of rotation of the storm.

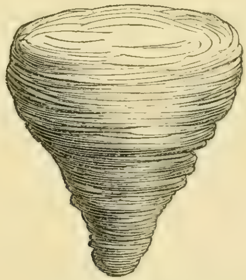
J. K. Marbourg, Jackson Township, Section 17, whose house was 80 yards from storm-centre, with an excellent view of Gibson's house, watched the storm a long time before it came. The west was first filled with clouds, which extended until they covered all the western and northern heavens, reaching a little beyond the zenith. The tornado first appeared as two clouds, one from the south-west and the other from the west rushing to one point. Together they presented somewhat the appearance of an arrow, thus:—

Fig. 24.



The whirl was seen forming when they met. Above them were dark, heavy clouds. When the tornado came nearer it presented the appearance of one funnel, revolving contrary to the hands of a watch, and drawing everything up.

Fig. 25.



This witness related what he saw as follows:—"When at Gibson's house, where I had the best view of it, and where it was 120 rods distant, it presented the appearance of two funnels uniting in one, at the height of 40 or 50 feet. The bases of the two funnels were about 200 feet apart:

they presented somewhat the appearance represented in Fig. 26. The two funnels did not appear to revolve around

Fig. 26.

each other. The first came to the east of Gibson's house, took his stable, and then turned back to his house. The two then appeared to unite. Could not see the two afterward. The tornado disappeared behind a building. It grew very dark. The funnels were of a dark blue; everything in them was rising. The timbers from Gibson's house flew up. Did not see clear sky through the open space between the two funnels, but a bright yellowish hue. The upper funnel extended to the clouds above. Hail, or rather chunks of ice, from a pigeon's to a hen's egg in size, fell before the tornado column came in sight. The wind then blew gently from the east. After the tornado had destroyed the school-house there came a violent gust of wind from the north-west, which considerably damaged my out-buildings. I had started to go to the school-house, and it carried me several yards before it. Immediately there fell a torrent of rain, with cold wind from the north. Was near where the school had been, but could not see anything. Suddenly saw the teacher and children, as if they had sprung out of the earth; they were coming toward me; they were shivering; they could give no account of what had befallen them; never saw such miserable-looking beings in my life. I had four children there, and did not recognise them. The mud was pelted into their skins, so that it could not be washed out; it is not all washed out yet. A dead child was found 40 yards north-west of the school-house. The storm oppo-

site my house was a mile wide. An oak post, 4 inches in diameter, was perforated by an oak board $4\frac{1}{2}$ ft. × 4 ins. × 1 inch."

David Canier, Jackson Township, Section 20, watched the funnel as it approached. It was perfectly dark in it. Could see boards flying out at the top of it. It was very large at the top and small at the bottom. It grew dark as night. Six persons took refuge in the cellar. The house went immediately like the clap of a hand, and the darkness was already gone. Then it turned pretty dark again, and rain fell in a sheet. It did not fall in drops. It was all over in a minute. Things in the cellar were not much disturbed. Bottles stood where they were.

Alexander Gibson, witness: Was at Mr. Canier's house. Hail as large as pigeon's eggs fell about twenty minutes before. Heard the noise about half an hour previous to the storm. Saw clouds coming from the north and south, and rushing together. Saw the funnel when three miles distant. Watched it when 30 or 40 rods distant. It was then as black as night, with boards flying around it. A very strong wind was blowing. It grew dark as midnight. Rushed down into the cellar, and was barely down when the house was struck with a sharp instantaneous rap, and in a moment it was gone. It was dark on going down into the cellar. It was pretty clear as soon as the house went. It then grew dark again, and a tremendous rain came down. Saw no lightning. Heard no thunder. A beam was driven right through a hog.

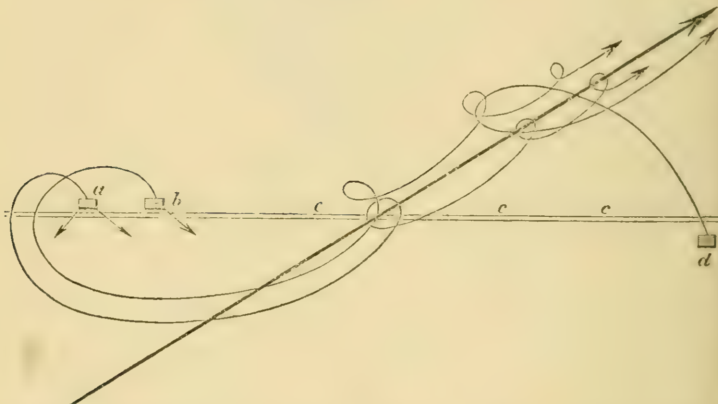
Mr. Canier's house was 24 × 28 × 14 feet to the eaves. It was pushed 18 feet due north without touching the ground. Its edge then came in contact with the soil and with tree roots, having fallen 2 feet from its original position, and the house toppled over, and was blown to fragments. The ruins were carried first to the north-west and then around to the north-east. Heavy oak sills were carried hundreds of yards and broken. The strongest iron-bound machinery was knocked to pieces and carried away. The sickle-bar of a Buckeye machine was carried 30 rods. A beam 14 feet × 6 × $6\frac{1}{2}$ inches was driven 3 feet into the soil in a slanting position. The beam weighed about 50 lbs.

A half mile to the west of Mr. Canier's house stood the school already mentioned and a house occupied by Henry Waters. Between these buildings and Canier's house, and 100 yards to the north-west of the latter, passed the storm-centre. The house occupied by Mr. Waters and the school-house were first blown to the north-west, and then in a circle

by the south round to the north-east. A hedge ran along the road which connects the houses. It presented the appearances already described.

The house occupied by Mr. Waters was first blown to the north-west, the sills remaining. The sills were afterwards blown to the south-east. The sills of the school-house had likewise, after being deserted by the house, been pushed to

FIG. 27.



Representation of Effects of the Storm at these Houses. *a* shows the position of the School. *b*, Waters's House. *c c c*, the Road. *d*, Canier's House.

the south-west, and, finally, the position of the *débris* among the ruins showed there had followed a violent wind from the north-west; thus a tree lay across the foundation of the school from the north-west.

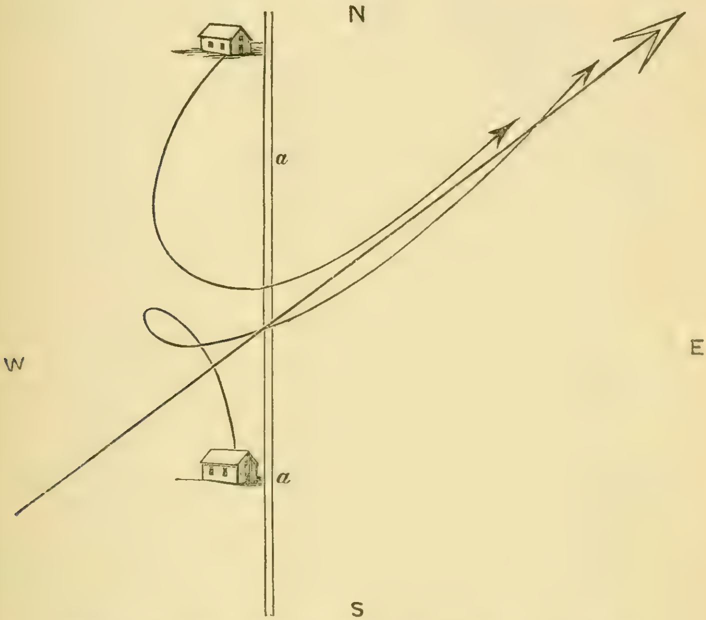
L. B. Babcock, son of J. P. Babcock, Jackson Township, Section 17, said that: Hail fell before the storm as large as pigeons' eggs. Ran to get into the cellar. Cellar door was blown over me as the house went. Came out of the cellar immediately and was blown south. The rain then came down in sheets with a north wind. The shutters from the house were carried 3 or 4 miles at least. Jacob Zek got only partly down the cellar stair and was hurt in the head.

J. P. Babcock's house was 34 × 26 × 14 feet to the eaves. The gable end faced the wind. The house was pushed bodily towards the north, crushing the northern foundation, tore up the ground a short distance, toppled over, and went to pieces. It stood on the south-east of the storm-centre.

Mr. Zek's house stands on the north-west edge of the storm. It was pushed from its foundations 12 feet to the S.S.W., ploughing up the ground. The windows on the north

side were smashed in. The windows on the south and east were blown out. A haystack was entirely blown away. It went, as could easily be traced, first S.S.W., and then round in a circle to the north-east. It went 40 rods south before turning east.

FIG. 28.



Sketch of the Effects of the Storm at Babcock's and *Zek's*. *a a*, the Road.

The distance between these houses was about half a mile. At this point the storm-path turned a little nearer north than north-east.

F. M. Curry, Jackson Township, Section 9, tenant of John Flack's house, said: The family went to the cellar. The house went south. No one injured. Eighteen pigs blown away.

The house was pushed 45 feet due south and then went to pieces. No explosion. It was situated to the north-west of the centre.

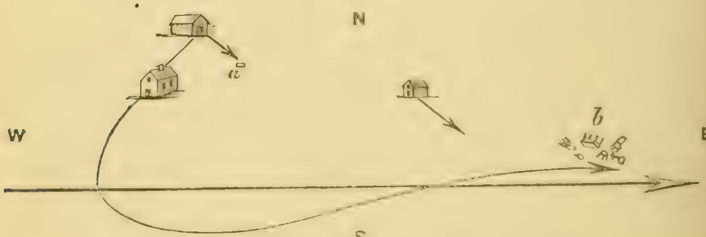
E. N. Wright, Jackson Township, Section 10: Was on the south-east side of the storm. Saw distinctly, at the distance of 2 miles, the tornado in funnel shape. The small end was down. Saw wood whirling. Heard its roaring after it went east. There appeared to be a mist or steam in

front of it. It rose and fell. Hail fell before the tornado, and an exceedingly violent rain after it.

Mr. Wright's house stands on the north-west of the centre of the storm-path, about 50 yards from it. A wood-house and a kitchen were blown south-west. The dwelling-house —a large stone structure—was moved 1 inch to the east. A stove weighing 450 lbs., and exposing to the wind a surface of 3 feet by 21 inches, was pushed 12 feet to the south-west, where it was stopped by obstructions. A bell, weighing 115 lbs., not under any circumstances exposing a greater surface than 1 square foot to the wind, and mounted 12 feet high, was broken from its fastenings, and carried 60 feet south. A granary, measuring 28 × 16 × 12 feet, and weighing, together with the grain, 55,000 lbs., was carried 21 yards to the south-east. After ploughing up the ground 2 feet in depth, it tumbled over and went to pieces. It was carried down a declivity, falling 6 feet. The width of the storm-track here was half a mile.

J. C. Cunningham, Jackson Township, Section 11: The house of Mr. Cunningham stood on the north-west of the centre of the storm-track, yet within the most violent vortex. The house was blown to the south-west; but the east door having been blown in the house went to pieces without tumbling over, and the floor remained upon the ground. The lighter furniture, &c., was carried far to the east. The granary stood to the north-east of the house. Its contents were emptied into the house cellar. The sills of the granary had remained where it was carried away. They were afterwards pushed to the south-east. The barn was blown to the south-east. A hog, weighing 200 lbs., was carried 500 yards. A cow, weighing 1000 lbs., was carried 200 yards.

FIG. 29.



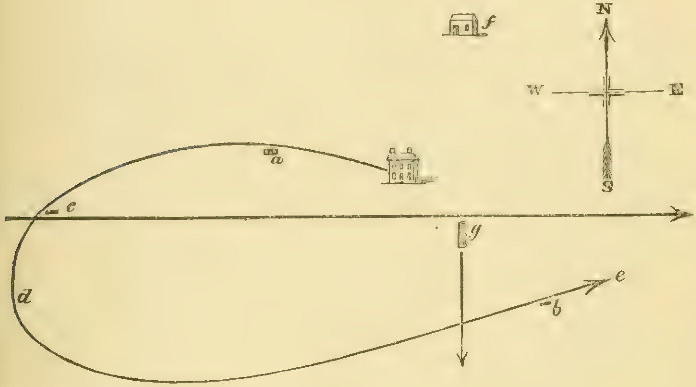
Plan of Ruins. *a*, Sills. *b*, Lighter Furniture.

There were four persons in the house and none were killed. The path of the storm is here 500 yards wide, and nearly due east.

D. T. Carringer, Jackson Township, Section 15: The house stood on the south edge of the storm. It was carried to the north-east 17 feet, going deeper into the soil as it went. It then turned over and was blown away in fragments. The straw among the trees showed that the last gust came from the north-west.

Another house struck was that of J. M. Davidson, Highland Township, Section 7. It stood precisely in the storm-centre, which here, as if conscious that this was its last victim in Iowa, seems to have exhausted its utmost violence. The house, 18 × 12 × 14 feet, was blown bodily from its foundations down a declivity towards a slough. It never touched the ground, but was scattered in the air. It seems to have been carried first nearly due west and then the

FIG. 30.



Plan of the Ruins. *a*, Davidson's Body. *b*, Housel's Body. *c*, Mrs. Davidson. *d*, Heavier portions. *e*, Lighter portions. *f*, Stable. *g*, Log.

lighter portions round in a curve to the east. The body of Mr. Davidson was found 20 yards west of where the house stood. The child was blown still further in the same direction. Mrs. Davidson was carried about 100 yards west. The body of Leyden Housel was found to the south-east. The heavier portions of the house were found some hundreds of yards to the west. Mr. Housel's body had evidently been carried west with the fragments of the house, and then back to the east. Mrs. Davidson and the child escaped with their lives.

A log of green water elm, 7 feet in circumference, 8 feet in length, and weighing at least a ton, was carried 50 yards straight south. A horse, weighing 1080 lbs., was carried 45 yards. A hog weighing 300 lbs. was blown 30 yards. The stable and barn were blown south. An iron plough,

with wooden handles, was carried 150 yards. The strongest iron-bound machinery was utterly destroyed. The young trees in the orchard were either barked or torn out. The storm was exceedingly violent for a breadth of 150 yards, but fences were blown down for nearly a mile wide. At the house of Thomas Davidson, a quarter of a mile to the north, the wind blew so strongly that the door had to be held. The north wind was evidently subsequent to the east wind, because the house could not have stood such a wind.

A. Davidson, Highland Township, Section 7, witness:—It commenced to rain about ten or twenty minutes before the arrival of the tornado. Shingles and other stuff began to fall with the rain. Then went out to see where they came from. Saw the tornado about 30 rods distant; it appeared to be coming toward me. Saw it bend to the south-east; it was funnel-shaped, and as black as possible; it whirled; thought it whirled with the hands of a watch; it passed 20 rods to the south of the house, and bent again to the north-east. When 30 rods distant from the house it suddenly disappeared. Afterwards saw another funnel, not reaching to the ground, travelling toward the north-east. When first seen it appeared to be only 60 feet above the ground at its lower end. The farther it went the higher it seemed to get.

Mr. A. Davidson's house is situated ½ a mile east of where J. M. Davidson's house stood, and a little north. The shingles and other material he saw falling came from his brother's house. The following sketch shows the direction

Fig. 30.

a, Davidson's House. *b*, Major Davidson's House. *c*, Disappearance. *d*, Fence.

of the storm-path after leaving Major Davidson's place until it ceased to touch the earth as a funnel; 20 rods south of A. Davidson's house it crossed a fence, exhibiting the same appearances as those already described. I was particularly careful upon this point, because of Mr. Davidson's

belief that the funnel whirled with the sun. I thought it possible that the tornado, before lifting from off the ground, might have changed its direction of rotation. But the evidence to the contrary was so striking that Mr. Davidson at once, upon my pointing it out to him, admitted that he must have been mistaken. He had not paid particular attention to the matter, and it was very dark when his observations were being made. The fence in question ran north and south. It was only thrown down for a space of 60 yards, and the boards were not carried away except in the middle. The posts remained in the ground, those on the north of the centre leaning west and those on the south leaning east. The posts in the centre were leaning some one way, some another. The funnel disappeared upon a knoll in the midst of a hollow about ½ a mile in diameter. There was no *débris* deposited at this point, nor were there any signs of a cataract of water having poured down. The wheat, however, was mown as low as possible, and the ground looked as if it had been baked, according to Mr. Davidson's statement. The funnel had narrowed to a point before it disappeared.

After leaving Mr. Davidson's the tornado cloud travelled to the south-east. Its progress was now difficult to trace, because of the little attention usually paid to a dark cloud, a high wind, and a rain storm. Considerable time had also now elapsed since the meteor passed.

The storm passed out of Washington County into Louisa County, travelling south-east.

Joshua Luckey, Louisa County, Union Township, Section 17, witness:—The path of fences blown down on my farm was about 600 yards wide. The fences on the west of the centre were thrown north; those on the east were thrown south. Some hail fell, and a smart shower of rain. Heard no roaring after it passed.

Charles Crim, Union Township, Section 20, witness:—Fences, 200 yards wide, thrown down to the south-east. The roaring was very loud for an hour previous to its arrival. Did not hear it after it passed. Saw no lightning. Saw a tongue of cloud shaped like a funnel hanging from the clouds; it did not reach the earth; at first it was hanging perpendicular, then it commenced whirling like the tail of a suspended snake.

At this point I lost track of the tornado, and could not recover it, although I spared neither time nor pains. The inhabitants of this county appear in general to be not nearly so intelligent as those of Washington County. Only now

and again could one find a man who could give any information even of what happened on their neighbour's farm a week or two before.

At this point I gave up the search, and took train for Illinois. Previous to describing its effects there the following testimony may find a place:—

H. C. Vittitoe, Warren Township, Keokuk County, about 3 miles north-west of the tornado, witness:—Saw a little south of the zenith the white under-clouds rushing in circles to a centre. The gyration was contrary to the hands of a watch. The funnel had not yet touched the earth. The wind came from the north-west pretty strong after the passage of the tornado, bringing with it a little rain and hail.

Dr. W. D. Hoffman, Sigourney, witness:—Mrs. A. T. Page collected a number of the largest hailstones. When melted, it was found that they had contained a quantity of twigs, leaves, dry grass, and mud, all reduced to fine proportions. Hailstones weighing from 4 to 8 ounces were common. One hailstone, which was shaped like an apple, measured $4\frac{1}{2}$ inches in diameter. The roaring was heard about twenty minutes before the hail began; it rose and fell like the cannonading in a battle. During the hail the wind came from the north-east, but it was very light.

Sigourney is 4 miles north of the storm.

Extracts from a communication received from R. L. Jay, Harper, Keokuk County, physician:—

"Was in the village of Baden, Keokuk County, German Township, 3 miles to the north-west of the storm-path.

"The tornado maintained an upright position. It moved rapidly at times, at others seeming to remain quite still. It was apparently about a quarter of a mile in height, and was funnel-shaped, and very dark and angry-looking. It whirled with the hands of a watch. There was a continual whirling of the clouds above the funnel: this was observed for some time after the storm had passed. Saw no lightning and heard no thunder. It was impossible to hear thunder owing to the noise of the storm, which was terrific. The direction of the wind was north-east until the storm had passed, when it changed to the north-west. Rain fell in torrents. Quite an amount of hail fell, of all conceivable shapes: three hailstones were picked up weighing $\frac{1}{2}$ lb. each. Half an hour after the storm the thermometer stood at 92°."

Extract from the "Sigourney News," May 28, 1873:—

"While the cyclone was mowing its path a few miles south and east of Sigourney, a tremendous hail-storm

visited this locality. We saw one hailstone which weighed 8½ ounces. The hail-storm was followed by a heavy rain, after which the sun came out bright and pleasant for the rest of the day."

Extract from the "Washington County Press" of May 28, 1873:—

"The writer was in a street car, half-way from Moline to Rock Island, at 4.30 P.M., on Thursday, when it was struck dead ahead by the most terrific wind and rain storm we ever experienced."

S. J. Mather, editor "Wilton Chronicle," Iowa, witness: Between 4 P.M. and 6 P.M. a very dark cloud passed over which poured down a very heavy rain.

Extract from the "Chicago Tribune," May 25, 1873:—

"At Muscatine the rain came down as if the flood-gates of Heaven had been opened, followed by hail, Between there and Wilton Junction, at Monona, Fredonia, and other stations along the south-western division of the Chicago, Rock Island, and Pacific Railroad, the country was flooded."

The tow-boat *Victory* lost her pilot house and smoke-stack when near Buffalo, on the Mississippi. The tug *Nonesuch* lost her smoke-stack and pilot-house at Moline. She was driven ashore.

A. H. Swan, editor of the "Monmouth Review," witness: There was an exceedingly heavy rain, a little hail and a great deal of lightning about 5 P.M. The atmosphere smelt as of brimstone previously. It was oppressively hot; perhaps 95°.

The temperature and pressure on May 22, 1873, at places near the tornado:—

	Davenport.		Keokuk.		West Union.	
	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.
7 A.M.	29·73	64°	29·64	69°	29·49	61°
2 P.M.	29·60	77	29·51	85	29·46	65
9 P.M.	29·69	66	29·71	72	29·53	64

The condition of the barometer and thermometer at West Union was obtained from Frank McClintock, correspondent of the Smithsonian Institute, who also furnishes the following noteworthy fact:—"Wind changed from the south to the west rapidly at 3.45 P.M. It worked back to the south before 9 P.M." The relative humidity at—

	Davenport.	Keokuk.
7 A.M.	94	85
2 P.M.	77	54
9 P.M.	94	85

We do not, therefore, overrate the relative humidity when we estimate it at 65 per cent at 2 P.M. The average temperature at the earth's surface in the line of the tornado at 2 P.M. was, from the above, probably 76°, but in the wooded hollows much greater.

Mr. Jay, however, of Harper, Keokuk County, states that the temperature was 92° half an hour after the storm. The day was generally described as being very warm.

Having arrived at Prairie City, Illinois, I endeavoured to find out the exact locality where the tornado first began to overthrow fences, damage buildings, &c. On the farm of James Williams, Point Pleasant Township, Warren County, there was a strong wind, but not sufficiently strong to prostrate fences. About 1 mile to the east is the farm of Israel Jared, Point Pleasant Township, Section 24. Mr. Jared testified as follows:—Saw streaks of cloud moving from the north and south toward each other before anything touched the earth. Saw a cloud in the form and about the size of a haystack strike the ground on my farm. A few minutes before hail of moderate size and in small quantity fell, followed, as the whirlwind was passing, by a smart shower of rain. The wind, which had been south-east, changed to the west after the tornado passed. It was pretty warm before, and cool after it. Heard roaring some five minutes previous to the arrival of the storm. Fences were blown down for 200 yards wide. They were blown toward the east. Saw some lightning. There was a heavy cloud to the north as the storm approached.

Mr. Jared's house stands about 200 yards north of the storm-path. The tornado was here travelling a little to the north of east.

John F. Tatman, Israel Jared's farm, testifies that he saw the funnel strike the farm, and that he heard the roaring for a long time previous.

Before leaving this farm the storm had developed all the characteristics of a tornado, except that the east wind was not yet powerful enough to destroy. The whirlwind then passed along a ravine full of tall timber. Nearly all the trees were uprooted or broken, but generally the latter. They appear to have had a firmer hold of the soil than those in Iowa. The breaking usually occurred about 6 feet from the ground, and the barking was almost entirely confined to the broken trees. The bark was torn off both up and down from the place where the timber was broken. The trees, when sound, were seldom broken clean across. Half or more of the wood was severed and the remainder was bent

so as to allow the tree to rest upon the ground. The broken timber, when sound, invariably presented the appearance of a broom. The fibres of each year's growth were loosened from the others and split. It was easy to obtain pieces 10 feet long and as thin as a wand. This separation of the fibres had evidently been brought about by the excessive straining and bending of the tree before it fell. Each year's growth had apparently been snapped asunder by itself, beginning at the outside. The trees all lay as they had fallen, being, when once down, protected by the surrounding timber. If they had, after falling, been subjected to a wind from another direction, so as to break them off entirely, they would have presented the precise appearances which have, by certain meteorologists, been attributed to the action of electricity. Many of these broken trees were 2 feet in diameter. When a tree, still standing, had its bark torn off at any point, an examination generally showed that the fibres of the tree were separated and perhaps partly severed at that point.

The path of the tornado was exceedingly well marked among the young trees. It could be followed by the eye as far as the ground permitted. They were torn and bare, while all around was green. This barking of the young trees took place within much narrower limits than those within which grown timber was overthrown. Generally it did not extend over twenty to thirty yards in width, and was sometimes much less and even disappeared altogether. The trees exhibited the marks of the greatest violence on the side from which the storm came. The twigs were smashed and broken off, the bark partially removed, and even the timber bruised. These results were evidently the effect of a rush of missiles against the trees.

The tornado at one point suddenly narrowed the path of extreme violence from 50 yards to 12 yards, at the same time changing its course to the south-east. This change of direction brought it toward Swan Creek, along the northern bank of which it had hitherto been raging. Just as it struck the creek the track was nearly S.S.E. Immediately thereafter it turned nearly due east, following the creek. The bottom of the ravine, within which Swan Creek flows, is about 200 yards wide, is surrounded by steep and lofty bluffs, and was covered with large trees. Among these trees the whirlwind raved with the utmost fury, developing an energy surpassed at no other point in its career. Trees from 3 feet to 4 feet in diameter were snapped or uprooted. Many large trees were broken off at a height of about 40 feet and left without

a twig. By far the greater number of trees along the ravine were thrown down from the south-west. The same broom-like appearance was generally presented by the broken trees as already mentioned.

Thomas Warmoth's house stood in the bottom of the ravine. Mrs. Warmoth testified as follows:—Heard a roaring first. When the storm came near the roaring was louder than thunder. Some hail fell just before the tornado blew. It was immediately preceded by a very bright flash of lightning. Went into the house and got upon my feather bed, together with my child, because I was afraid of the lightning. A large tree was blown down, catching the side of the house. The house went to pieces, the tree, however, keeping the floor in its place. Found myself under the feather bed with my child. The bed was pinned to the earth by pieces of timber. Was soaking wet. Everything was covered with mud. Heard no thunder. The lightning struck a tree, depriving it of its bark.

I was unable to find this tree. Mrs. Warmoth's testimony is valuable as a curiosity. It was generally impossible to obtain any information from the ladies. If one questioned them rigorously they took it as an insult, and if allowed to tell their own story they immediately commenced running such a muck among the prodigious and the incredible that one was glad to make his escape. In this connection it is also worth mentioning that although invariably offering payment for any necessary hospitalities, I soon learned the wisdom of always addressing myself to the master of the house when asking for such.

After leaving Warmoth's house the tornado-centre crossed to the south bank of the creek, where it continued for nearly a mile farther, although somewhat increasing its distance from the creek. It crossed to the south bank where a smaller creek joins Swan Creek from the south. This brought it nearer to the house of A. J. Caton, Swan Township, Section 15, which stands about 500 yards from the centre of the whirlwind. Part of the roof was blown to the south-east and part to the north-east. A smaller house was blown a few yards to the north-east and inverted. A house on Mr. Caton's farm, tenanted by N. J. Reynolds, 14 feet by 20 feet and one story high, was carried 8 yards to the north-east bodily. It then struck the ground, tumbled over, and was blown to fragments. Its four inmates were carried from 50 to 60 yards, but not killed. It stood a little south of the centre. After passing Mr. Caton's farm the tornado entirely left Swan Creek.

After leaving Swan Creek the storm travelled E.S.E. The first house which came in its way was that of Absolom Vandevere, Swan Township, Section 15. He testified as follows:—I have reliable information that two or three miles to the north the clouds were seen rushing south. The clouds came also from the south toward the tornado. Streaks of unusually strong wind seemed to come now and again from the south side and run into the main whirl. The wind on the north side was not nearly so strong as on the south side. Twice as much fence was blown down on the south as on the north side. Its noise resembled that of machinery, only very loud. The dimensions of my house were 36 × 42 × 18 feet.

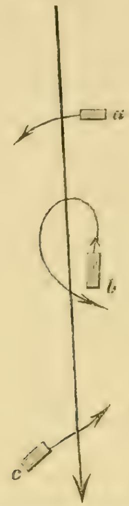
Samuel Larkins, Swan Township, Section 15:—Was at Mr. Vandevere's house when the tornado struck it. Heard roaring about 15 minutes before it came. When I first saw the funnel it did not touch the ground. Saw it whirling contrary to the hands of a watch, and the clouds were drawn in toward it on all sides. It did not lighten before the storm. It lightened a great deal immediately after it in the west. It did not hail nor rain. Did not see clouds in north or south. There appeared to be only a narrow strip of clouds. A McCormick reaper, weighing probably 1000 lbs., was carried ten rods from the south. Two horses were blown, the one 50, the other 40 yards. An axle-tree, torn from a waggon, was carried a mile and a quarter to the south-east. A shingle was driven through a half-inch ash board. It is in the possession of a Mr. Thomas. A rafter 8 feet × (2 × 4) inches, was driven through three hogs and thrust into the ground a foot and a half. All three were on it when it was found. A picture-frame was picked up with the glass unbroken. The wind blew very strongly for about two minutes.

Mr. Vandevere's house, 36 × 42 × 18 feet to the eaves, stood north and south lengthwise. It was moved due north half the length of the house, tumbled over and blown to pieces. It stood with its north end exactly in the centre of the vortex. There were nine persons in the cellar and two in the house. One of those in the cellar was killed by a log. The trees around the house and within the narrow path of the greatest violence, all pointed to the E.S.E., the direction in which the storm was travelling.

W. J. Jones, Swan Township, witness: Vandevere's house is forty rods to the north. Myself and wife were in my house when it was blown away. It grew dark as midnight just then. There was a little hail before. Did not notice any rain. The house did not go very fast.

Mr. Jones's house was 22 × 16 × 12 feet to the eaves. It was pushed entire 5 feet to the north-east. It then toppled over and was blown to pieces.

Fig. 31.



Sketch of Storm Effects at Larkin's, Vandevere's, and Jones's Houses.
a Larkin's House; *b* Vandevere's; *c* Jones's.

The fragments in Illinois had already been gathered from the fields, rendering it more difficult to trace the ruins in their flight.

William Thomas, Swan Township, witness:—The centre of the storm passed a quarter of a mile to the north. An unoccupied house stood in its way. It was lifted from its foundations and then broken to pieces. Horses were carried a good way and killed. A rail was driven through one of the cattle, going in beneath her tail and coming out at her shoulder. Saw only one funnel.

Such is a statement of some of the facts connected with the Iowa and Illinois tornado of May 22, 1873. I spared no pains in order to render it scientifically complete, sometimes travelling miles under a fierce sun, and with a temperature among the nineties, in order to obtain the evidence of one man. The information given by any witness by no means represents the number of questions asked him. These were extensive and calculated to extract all the knowledge on the subject possessed by those under examination. For instance, the following question was addressed

to all and sundry: Did you observe any pointed objects, such as lightning-rods, posts, &c., tipped with flame, during the progress of the tornado? but the replies being uniformly in the negative, have not been formally inserted in the statement of facts. Also a description of the sound was exacted from all witnesses, but only a few typical ones have been inserted. I regret this at present, because I have learned from experience that very important questions may attach themselves to a description of the sound. While interrogating parties the utmost vigilance was exercised to prevent them from giving conclusions for what they saw and heard. This was a very troublesome point and caused the interrogator to appear in many cases in the highest degree rude; while it also excludes from these pages the names of persons who observed accurately, but who are unable to distinguish between the *esse* and the *ergo*.

The statement of facts for Iowa is much more exhaustive and instructive than that for Illinois. There are several reasons for this.

1. Some weeks had already elapsed since the tornado, and its traces were becoming rapidly obliterated, both in the memories of the witnesses and upon the surface of the earth.

2. It was later in the day when it occurred, thus hiding the light of the sun more completely by the tornado clouds as they approached, and rendering it more difficult to observe accurately their forms and proportions.

3. The storm in Illinois seems to have been of a somewhat different character from that in Iowa, by its form rendering observation more difficult.

With regard to the angles of elevation given, it must be borne in mind that they are only approximations. Very few indeed of the witnesses have accurate conceptions of angles. I generally made them point in the direction in which an object was seen, and so estimated the angle.

Calculations and Conclusions.

The Idea of a Tornado in General.—The tornado consists essentially of a rapidly ascending current of air. This involves two other functions—(1) a rushing in of the air at the under part of the ascending current or column; (2) an out-rushing at the upper. Upon the former of these functions, combined with modifying circumstances, depends the peculiar character and career of the under currents and of the clouds they bear; upon the latter, combined with the same circumstances, the proportions and direction of

motion of the upper currents of the heavy masses of clouds they bear. There appears to be nothing in the nature of the tornado itself which can determine the motion of either the upper or under current more towards any one point of the compass than toward the others. This direction of motion relative to the ascending column depends upon the direction and velocity of motion of the latter and of the atmospheric strata in which the influx and efflux take place, modified to some extent by the differing velocities of revolution of the surface of the earth at different parallels of latitude, by the form of the earth's surface, and by the variation in the constitution of the atmosphere. If the tornado column, and the atmospheric strata which it penetrates, move in the same direction and with the same velocity, the influx and efflux will take place in nearly equal quantity on all sides of the column. If they move with different velocities the directions of exaggeration and diminution of the influx and efflux can be calculated in the same way as the direction of a wind-vane on a ship's mast, given the directions and velocities of the motion of the wind and of the ship.

General Idea of the Tornado of May 22, 1873, in Iowa.—A huge, dark cloud covered an area at least 30 miles in diameter. Under the south-west edge of this cloud there moved a perfectly opaque funnel-shaped appearance, reaching from the ground to the clouds. Towards its base the wind, in spirals, rushed violently from all sides, overthrowing, when in immediate proximity to or within the opaque vortex, whatever opposed its progress. Towards its summit, where it disappeared in overhanging horizontal cloud, long streaks of clouds rushed in spirals from all directions. Viewed from a distance they appeared to come from opposite directions, and move swiftly towards each other at right angle to the observer's line of vision. Between the surface current and these centripetal clouds the air doubtless obeyed the same forces, and rushed in spirals with ever-increasing velocity towards the opaque funnel.

Under the remainder of the lofty cloud which defined the limits within which the outspreading of the ascending air was taking place, and which lay chiefly to the N.E. of the rising column, there raged a tremendous storm of hail and rain, accompanied by incessant and brilliant electrical phenomena.

More Particular Description, Dimensions of the Meteor, and of the Centrifugal or Upper Current.—The data for determining the horizontal extent of the cloud are not very precise. There is a general concurrence of the witnesses that

it reached only a few miles to the south of the tornado, and that very little rain or hail fell there.

There is also a general agreement among the witnesses that a little hail fell some minutes before the advent of the funnel, and a little rain immediately after its passage. The whole appears to have occupied at least thirty minutes in passing. This would give the cloud at the funnel a width of at least 15 miles in the line of its progress. Probably it was considerably greater. We may, therefore, safely conclude that we are within the limits where we assign to the centrifugal cloud an average horizontal diameter of 30 miles.

I have no data for determining the elevation of the under side of the centrifugal cloud beyond what is involved in those for measuring the altitude of the centripetal. There must, however, have intervened a very considerable space between the two. For if there was nothing intervening there could be nothing to prevent the under, and specifically lighter, air from taking the shortest way up. There could be no reason why it should first rush to a centre, ascend there, and then rush from it.

Dimensions of the Centripetal Current.—The data for determining these are still less precise than the preceding, but yet sufficiently so as to give a more definite idea than any mere description in general terms.

The witnesses generally testify that hail fell from fifteen to thirty minutes before the whirlwind, and rain for as long a period after. They further almost unanimously say that the wind was easterly with the hail and westerly with the rain. If we assume twenty minutes as the duration of each of these winds, we obtain 20 miles as the diameter of the inrushing winds at the earth's surface.

If the disturbing influences were not much greater at the earth's surface than at a higher altitude, the dimensions of the whirl should, within the limits of the centripetal wind, owing to the absence of friction, vastly increase as we ascend. Moreover, since the funnel was first formed at a considerable elevation, and since it touched the ground with a narrow point, and merely incidentally, as it were, since it for long distances ceased to strike the earth, and yet proceeded with undiminished energy; since, in short, both the originating and sustaining sources of its power seem to have mainly existed high in the atmosphere, we have the strongest reasons for concluding that the horizontal dimensions of the centripetal current was very much greater at its more elevated portions than at its base. It would therefore appear to be not unlikely that the diameter of the

centripetal current was not greatly exceeded by that of the centrifugal.

The Dimensions of the Funnel.—The data for calculating the dimensions of the visible portion of the funnel are more numerous than precisely accurate; but I conclude that the tornado column was, from a favourable position, visible at some parts of its course to a height of between 1 and 2 miles.

Wherever there was evidence of the funnel having touched the ground there was, to a greater or less breadth, what I have designated as the vortex of extreme violence, varying from 100 yards in breadth to nothing. Without having positive evidence, and notwithstanding that there was everywhere a gradual diminution of violence from the centre to the circumference except in the remarkable phenomenon of streaks or arms, and no abrupt transitions, I could not help concluding that the path of greatest violence was identical, or nearly so, with the diameter of the base of the funnel when it touched the ground. This would give it an average diameter of about 30 yards. The witnesses are generally agreed that the summit was several times wider than the base. If we assume that the visible top was five times the diameter of the base, we have, say, for the altitude of 1 mile, a diameter of only 150 yards. I think, however, that, owing to its great elevation and the optical delusion connected therewith, and the general belief of the spectators, that the funnel was, at the very utmost, only a few hundred feet high; the diameter of the summit was very much greater.

The Changes seen in the Funnel.—Some observers saw only one funnel, others saw two funnels superimposed with the narrow ends together, and the smaller one beneath, while a great many saw two and even three funnels side by side. The evidence on all these points is beyond question. At first I was inclined to believe that two or more funnels had actually touched the ground and travelled side by side. The sketch of the storm by R. F. Campbell seemed to favour this explanation. It shows a protuberance on each side of the funnel, which looks like an inceptive funnel. The fact that a second funnel should travel along the south-east side of the main one without disturbing the symmetry of the action of the latter, as exhibited by the ruins, did not appear altogether incredible, for it is abundantly evident from the statement of facts that the damage was done by a wind blowing in the path of the tornado when the black funnel hovered above or slightly touched the ground. Thus an incipient funnel, moving along the south-east side of the

main funnel, and occasionally making a dip, would produce no appreciable discord in the disposition of the ruins. An insuperable objection to this theory was found in the unanimous testimony of the eye-witnesses that the funnels approached each other and combined to form one. Another objection was found in the lack of unity thus introduced into the conception of the magnificent whole. Finding this explanation untenable, I endeavoured to think the possibility of smaller auxiliary funnels, each of them a perfect whirl, moving in spirals nearly or altogether identical with those pursued by the confluent winds. This I found to be more difficult than the first. The chief objection was the total lack of a conceivable cause of the existence of these smaller whirls. The second was the peculiar circumstance that while one observer saw two funnels, another in the same position saw only one, or saw two superimposed with the smaller ends together. No possible arrangement of two independent funnels with the smaller ends down could produce the optical illusion of the funnels superimposed with the smaller ends together. I found what I believe to be the key to the difficulty given by Mr. Marbourg while considering the delineation of the two funnels joining in one funnel. The other observers who saw two or more funnels evidently had the whole of their attention confined to these, and did not observe what was above them. He saw the two combine at what he estimated an altitude of 50 feet, and form one. Another witness at the same place and time saw only one funnel. At Lancaster some saw only one, others saw two superimposed, and others saw two side by side and all at the same time. There is, therefore, an unavoidable necessity for some explanation to reconcile these antagonistic appearances.

There is excellent evidence to show that the funnel moved at its base with a sort of pendulum motion; that it seemed to stand still for a moment and then to bound suddenly forward. The evidence on this point is so general that we need not recapitulate it. But we have no reason for supposing that this oscillating motion extended to any considerable distance above the ground, for it is only in the want of homogeneity of the atmosphere and the resistance opposed to the free course of the winds by the earth's surface that we can find a cause for this pendulum motion. Let us suppose that the funnel is over a well-watered and well-wooded ravine, with its path at right angles to it. It is evident that the air at some distance above the ground will offer much less resistance to the forces drawing it into

new paths than that on the surface when the friction is so important an element. In the forward march of the tornado the base would thus be somewhat left behind. The warm, moist air of the ravine would increase this tendency on account of its specific levity.

But the inrushing winds follow the path of least resistance toward the area of greatest rarefaction. The bending, therefore, cannot proceed far before the south-west winds, whose direction of motion is carrying them away from the base of the inclined funnel, will find less resistance and a shorter path towards the greatest rarefaction by striking up into the nearest point in the side of the funnel.

A streak of rarefaction great enough to produce condensation is thus generated. This arm of the funnel affording the shortest way for the winds will increase with rapidity. The other will decrease, becoming mainly confined to the north-west winds. Meanwhile the intervening air will become rarefied and set in motion; the two arms will suddenly unite, and the funnel present again its original proportions. These transformations must take place within a brief space of time, for the tornado is travelling at the rate of half a mile per minute. When we consider, in addition, the darkness, we shall be at no loss to conceive why these two branches should present the appearance of one funnel with its larger end down, and why to a less favourably situated or more superficial observer the whole should appear as one.

One difficulty yet remains to be obviated. Since the arm moves forward in a spiral to the centre, and the velocity of the inrushing winds at the funnel far exceeds 100 miles per hour, how is it possible for the observer in the darkness to notice the appearances distinctly? Mr. Marbourg gives the distance between the two arms at 200 feet. This space would be traversed in one second by winds of so great velocity, and the condensed vapour would fill the whole space before it had time to disappear. The very velocity itself would render distinct vision difficult. We must not, in short, confound the path of the winds with the winds themselves. While the winds were rushing up in a slanting and curving direction along these arms, the arms themselves might be moving forward with a much less rapid motion. If it were possible to determine the inclination of these arms to the perpendicular it would aid in calculating the force of the wind. Mr. Marbourg stated that the distance between the bases of the two arms was 200 feet, and the height, when they joined together, 50 feet. He gave 80 to

100 feet, however, as the height, to the editor of the "Washington County Press." His own delineation formed the belief that the height was greater than 50 feet. He could easily judge of the distance between the arms, but was very liable to be mistaken in estimating the height. The error, therefore, will probably not be great if we assume that the funnels joined together at the height of 100 feet. This would give these arms an inclination of 45°.

The Direction in and the Velocity with which the Tornado Travelled.—From its starting-point on South Skunk river until it reached North Skunk river, a distance of 10 miles, the tornado travelled in an E.N.E. direction. It there turned a little south of east and followed the course of that river for 2 miles. It then went E.N.E. until it came to Rock Creek, when it turned a little west of north and followed the course of the creek for a short distance. It then travelled north-east until it ceased to touch the ground 3 miles from Keota. After lifting itself from the ground it travelled about E.N.E., striking again at Westchester, Washington County. From this point until it reached Enoch Wright's, a distance of over 9 miles, its course was nearly straight, varying between N.E. and E.N.E. Its course then became very crooked, bending in curves from north-east to south-east, and *vice versâ;* but on the whole travelling east, until it came within a mile of the confines of Washington County, when it made a decided turn to the south-east. Here its path lay down the declivity towards the well-wooded ravine of Goose Creek. When within half a mile of the creek its traces became so feeble that it was impossible to track it further. It was at this point, within 1½ miles of Iowa river, that the crookedness of its path reached its maximum within the last half mile of its career, before ceasing to touch the ground, as a black funnel at A. Davidson's farm, Highland Township. Within this half mile it bent just a little to the south-east, then to the north-east, then to the south-east, and again back to the north-east, when it disappeared.

After leaving Crooked Creek, at the commencement of its career in Washington County, and until it reached to near Goose Creek, the tornado traversed an elevated, well-cultivated region, almost totally devoid of trees and water-courses. There was apparently nothing in the configuration of the ground over which it passed which could account for its changes of direction. These changes, moreover, were not generally abrupt, the tornado sweeping in graceful curves from one direction to another. It is to be noted,

however, that the curving of its path commenced after it came within the influence of the moister warmer air of the Iowa river, and that the general deviation of its course from the north-east direction, which it had on the whole hitherto followed, led it down the valley of the Iowa river.

In order to determine the velocity with which the meteor travelled, it was necessary to obtain the precise time of its arrival at different points. The difficulty of obtaining precise time in an agricultural district is always considerable. In addition, no one thought of looking at his clock or watch. Hence, although every witness was questioned on this point, there were but few who could even approximate to the time. The majority thought that they had a remarkably definite idea when they could tell between what hours it occurred. Reliable time it is believed was obtained at least at two places in Iowa—at Wolfden school-house, Lancaster Township, Keokuk County, and at A. McKee's house, Section 23, Cedar Township, Washington County. The times obtained at these places were corroborated by the testimony of those likely to be best informed. The tornado struck the school at 2.15 P.M. It arrived in Section 23, Cedar Township, at 3.10 P.M. The distance between these places in a straight line is 27 miles. This gives a velocity of 29·4 miles per hour. The crookedness of its course, though not great, would bring its velocity along its path to 30 miles per hour at least.

The Direction of the Centripetal Winds.—The wind blew in spirals toward the centre of the vortex, the direction of revolution being contrary to the hands of a watch throughout the whole course of the storm. The evidence in support of this, as given in the statement of facts, is overwhelming, and recapitulation unnecessary. The mere fact that all who had an opportunity of seeing the funnel saw that it was circular is sufficient to prove that it was a whirlwind, for on no other hypothesis could the circular form be accounted for. Then the witnesses, almost without exception, saw this funnel whirling contrary to the hands of a watch. Then the illustrations of the position of the ruins of the houses, and of the fallen trees, &c., prove beyond a doubt that a merely centripetal wind could not have done these things. Again: that the wind was not blowing in circles round the centre is sufficiently demonstrated by the fact that everything, except within the most violent vortex, was thrown toward the centre; the ruins invariably lay most thickly there. The only form of motion capable of producing these effects is a mean between the circular and the direct centri-

petal,—to wit, the spiral. The illustrations give copious ocular demonstration that such was the case. The ruins of houses, whenever they could be distinctly traced, proceeded in the curve towards the centre, which apparently was always reached before the completion of an entire revolution. This does not imply that the centripetal winds never performed a complete revolution before reaching the centre, but only that from within about 100 yards of the centre this did not take place. Ruins carried from that distance seemed generally to reach the centre in about two-thirds of a revolution. This led me to assume at about 100 yards from the centre the wind generally made an angle of about 25° with the tangent. This gives a centripetal component compared with the circular as 0·42262 is to 0·90631.

Besides its horizontal motion, the wind had necessarily an ascending motion, which reached its maximum at the centre and its minimum at the circumference. The necessity for this lies in the proof of the centripetal motion of the wind. Otherwise the centripetal motion of the wind would require to double itself at half the radius from any point toward the centre : this would soon give it a motion infinitely great, which is absurd ; there would also be no means of escape for it at the centre. To a great many excellent witnesses the funnel presented the appearance of a screw with its thread running upwards. In addition, all objects within the sphere of the tornado seem to have been partly lifted, partly pushed forward. We have seen that at A. Gibson's house the arms made an angle with the perpendicular of probably about 45°. Since these arms, as there is every reason to believe, were inclined in the normal direction of the winds, which is that of least resistance, we are justified in assuming that in the black funnel itself the inclination of the winds to the horizon was at least as great, and probably greater.

The Velocity of the Wind.—This is a very difficult topic ; the task would have been much easier if we had been dealing with a horizontal wind merely. If even the perpendicular and horizontal components were invariable, the hope of a tolerably accurate solution of the problem would be much greater ; but while the former decreases from the centre to the circumference, the latter increases. In order that no chance of a solution might be lost, I give the following from data I collected :—

1. A school-house, weighing 30,000 lbs., was pushed 25 feet from its foundations. The surface exposed to the wind = 360 square feet + the roof, which had a slant of 45°.

2. A house, exposing 280 square feet to the wind, having a floor of 480 square feet, and weighing 20 tons, was pushed 6 feet from its foundations. It was then lifted over the tops of trees 20 feet high.
3. A house, presenting to the wind a surface of 176 square feet without the roof, and weighing 10 tons, was carried 24 yards while falling 3 feet. The wind struck it nearly on the end.
4. A granary, exposing 128 square feet of surface, and weighing 60,000 lbs., was pushed 14 feet.
5. A house, exposing a surface of 392 square feet, + the slanting roof, and weighing 20 tons, was carried forward 18 feet while falling 2 feet.
6. A house, exposing 533 square feet, and weighing about 25 tons, was pushed from its foundations.
7. A granary, exposing a surface of 256 square feet to the wind, and weighing 55,000 lbs., was carried 21 yards while falling 6 feet.

If we estimate the static friction of a frame house upon a stone foundation, or upon posts firmly fixed into the ground, and to which it is nailed, at one-half the weight of the house, it does not seem likely that we shall err by excess. Upon this basis it is easy to calculate the minimum velocity of a horizontal wind necessary to push it from its foundations. The difficulty is that we are dealing with a wind having a perpendicular component to its velocity. If the wall of the house was perfectly smooth, and since the air is nearly perfectly elastic, the only force exerted upon it by a wind blowing at any angle to its surface would be perpendicular to that surface. Moreover, whatever force the perpendicular component may exercise upon the wall, owing to its roughness, must have upon the house the effect of an overturning rather than a lifting power. But, since friction is independent of extent of surface, this overturning power would not assist in overcoming friction. The fact, also, that the houses, while being carried bodily through the air, were not made to spin round on an axis by this perpendicular component,—there being then no resistance, beyond inertion, to motion in a circle,—would seem to prove that its effect was not great. It would therefore appear that an approximation may be made to the velocity of the horizontal wind, by simply calculating—upon the given assumption of the relation of weight to friction—the minimum force required to push these houses from their foundations. The following are the results for the given cases :—

1. Pressure per square foot 41·7 lbs. Velocity of wind, 91·3 miles per hour.
2. Pressure, 71·4 lbs. Velocity, 119·5 miles.
3. Pressure, 56·8 lbs. Velocity, 106·6 miles.
4. Pressure, 234·3 lbs. Velocity, 216·5 miles.
5. Pressure, 51 lbs. Velocity, 100·9 miles.
6. Pressure, 46·9 lbs. Velocity, 96·8 miles.
7. Pressure, 107·4 lbs. Velocity, 146·6 miles.

Only one of these buildings was not, in addition to being pushed from its foundation, carried away, viz., case 4. It, however, was pushed through a dense mass of tough rubbish, and its sides were bent from the perpendicular. Owing to its small surface and great weight, the force of the wind exceeded the minimum required to push the building from its foundations less in this case than in any of the others. Hence we have the great velocity of 216·4 miles per hour, or of 317·4 feet per second, in a horizontal direction. It is also probable that in this case the kinetic friction was greater than the static, and that it increased every moment until the building came to a stand.

The Amount of the Precipitation.—It was impossible to obtain more than generalities on this head. There were no rain-gauge measurements, so far as I could learn. At Davenport, however, a few miles from where damage was done on the Mississippi, there fell, on May 22, 1·35 inches, as shown by the Signal Service Reports. From the description of the rain to the north of the funnel, in Washington County, a greater quantity than this must have fallen at many places. It is not, therefore, likely that we shall overestimate the amount when we assume that 2 inches of rain fell over an area 10 miles in width. Since the tornado travelled 30 miles per hour, this gives a rainfall per minute of—

$$\frac{2 \times (10 \times \frac{1}{2}) \times 3{,}097{,}600 \times 9 \times 144}{1728} = 23{,}232{,}000 \text{ cubic feet.}$$

Tornado Dynamics.—A cubic foot of water weighs 1000 ounces. The weight of water which fell per minute was therefore—

$$\frac{23{,}232{,}000 \times 1000}{16} = 1{,}452{,}000{,}000 \text{ lbs.}$$

It is not, probably, an exaggeration to assume that this mass of water fell, on an average, 6 miles or more. This gives a horse-power of—

$$\frac{14{,}522{,}000{,}000 \times (6 \times 5280)}{33{,}000} = 1{,}393{,}920{,}000,$$

generated by the falling water.

From the calculations of the velocity of the wind it is not probable that, when we assume—at the distance of 100 yards from the centre—an average velocity of 120 miles per hour, we overrate it. The angle which the wind made with the tangent was probably about 25°. The wind, therefore, on these bases enters a cylinder of 100 yards radius at the rate of 50 miles per hour, or 4400 feet per minute.

From the calculations of the height of the centripetal current we have seen that it probably extended over 2 miles. Let us assume that its height was 2 miles. Let us assume, also, that the velocity with which the wind entered the cylinder was equally great throughout its length. We have then the following data for calculating the quantity of air which enters this cylinder of 100 yards radius, or 628,318 yards circumference per minute. It amounts to $(628{,}318 \times 3) \times 4400 \times (2 \times 1760 \times 3)$ c. ft. $= 87{,}582{,}502{,}656{,}000$ cubic feet. Since this volume is calculated from the mechanical effects or force of the wind, and since the relation between the force and the velocity of the wind is calculated for the average pressure, it matters not, in calculating the mass of the above volume of air, what the barometric pressure was at the distance of 100 yards from the centre. The difference of pressure due to elevation must, however, be calculated. Since the average pressure at sea-level is about 30 inches, at an elevation of 1 mile about 24·60 inches, and at an elevation of 2 miles about 20·20 inches, and taking into account the correction due to the elevation of the country over which the tornado passed, 24 inches would be about the mean pressure up to 2 miles of elevation. The density of the air varies with the temperature. The temperature around the tornado must have been, at a fair exposure, about 76°, but was in deep wooded ravines no doubt much greater: since the temperature diminishes 1° for every 300 or 400 feet of elevation, we may assume the average temperature to have been, within the limits of the centripetal current, about 60°. The relative humidity was about 65 per cent.

The weight of a cubic foot of air, at a pressure of 24 inches, a temperature of 60°, and a relative humidity of 65 per cent, is 426 grs., or thereabout. The total weight of air, therefore, entering the column per minute amounts to—

$$\frac{87{,}582{,}502{,}656{,}000 \times 426}{7000} = 5{,}330{,}020{,}875{,}922 \text{ lbs.}$$

This weight of air moves, by hypothesis, at the rate of 120 miles per hour, or of 176 feet per second. This velocity is acquired from the acceleration due to gravity in falling

through a space of 481 feet. The power, therefore, generated by this mass of air moving with the given velocity amounts to—

$$\frac{5,330,020,875,922 \times 481}{33,000} = 77,689,092,161,166 \text{ horse-power.}$$

The latent heat of evaporation amounts to about 1000°. Consequently the condensation of 1,452,000,000 lbs. of vapour is accompanied by the evolution of 1,452,000,000 × 1000 calorics or pound degrees of heat. The specific thermal capacity of air, as compared with water, is as 0·237 to 1. The number of degrees, therefore, which this amount of heat would raise 1 lb. of air is—

$$1,452,000,000,000 \times \frac{1000}{237} = 6,126,582,278,481 \text{ degrees.}$$

But the pressure remaining the same air expands $\frac{1}{491}$ part of its volume for every accretion of 1 degree of temperature. This amount of heat would therefore expand 1 lb. of air $\frac{6,126,582,278,481}{491} = 12,477,764,315$ times. A cubic foot of air at 30 inches pressure, 60° temperature, and having a relative humidity of 65 per cent, weighs 538·6 grains. A pound of such air, therefore, occupies—

$$\frac{7000}{538 \cdot 6} = 12 \cdot 99 = 13 \text{ cubic feet very nearly.}$$

A pound of air, therefore, in doubling its volume must—the pressure remaining 15 lbs. per square inch—raise a weight 13 × 144 × 15 = 28,080 lbs. through 1 foot. The total amount of work, therefore, performed by 1 lb. of air, while expanding—under a pressure of 15 lbs. per square inch—to 12,477,764,315 times its original bulk, is 12,477,764,315 × 28,080 foot-pounds. This amount of work accomplished per minute demands—

$$\frac{12,477,764,315 \times 28,080}{33,000} = 10,617,443,089 \cdot 8 \text{ horse-power.}$$

We thus see that the power generated by the condensation of the amount of vapour estimated as having been precipitated is much less than that calculated to have been produced by the velocity of the centripetal winds. Theoretically, this power should be diminished by the amount of expansion counteracted by the removal of so large a mass of vapour. Practically, however, it seems probable that this removal of vapour, while increasing the specific gravity of the air, serves rather to increase the rarefaction when taking place with great rapidity.

The horse-power required to raise 5,330,020,875,922 lbs. to a height of 5 miles per minute amounts to—

$$\frac{5{,}330{,}020{,}875{,}922 \times 5 \times 1760 \times 3}{33{,}000} = 4{,}264{,}016{,}700{,}737{\cdot}6 \text{ horse-power.}$$

These figures convey a better idea of the tremendous power of the tornado than any mere verbal description could do. They also show that the power evolved by the condensation of vapour, while enormous, is by no means sufficient to supply the whole energy developed by the tornado. We must therefore look for another source of power. This is no doubt to be found in the destruction of the atmospheric equilibrium by an abnormally warm southerly current flowing under a much colder, and consequently specifically heavier, stratum of air.

In an extensive paper, which will be found in the Chief Signal Officer's Annual Report for 1873, the author passes on to the consideration of the general atmospheric conditions under which the tornado originated. The length of this article, however, precludes our entering fully into this branch of the subject.

The Electrical Phenomena must be regarded as merely secondary. In the lofty centrifugal cloud there was incessant thunder and lightning; but very little, if any at all, in the centripetal cloud and the funnel. One object, a tree, was struck in the path of the storm, but the discharge took place half an hour before the arrival of the funnel. The lightning-rod said to have been melted on widow Dogget's barn might have been struck long before, and only noticed then because it was blown down.

There were no evidences of electrical action upon the trees, which were peeled and shivered by the storm, although there was that brown appearance when the trees were broken which has been referred to the action of electricity. This, however, was clearly due to the straining of the tree before it broke.

No one saw any objects tipped with electrical light during the progress of the meteor. The telegraph was not interrupted until the wind blew down the wires.

It is the generally-accepted opinion that in tornadoes houses are frequently burst asunder when the centre of the whirlwind comes over them. This is supposed to be accomplished by the great rarefaction, which is said to exist in the centre, suddenly relieving the air within the houses of a large portion of atmospheric pressure. The air thus relieved of

pressure immediately expands and blows the house asunder, as with gunpowder. There was not, so far as I was able to learn, a single case in which a house was so destroyed. The houses were invariably demolished or blown from their foundations by a wind blowing in a certain direction. They were generally pushed or carried a considerable distance bodily, floor and all. After striking the ground with the foremost edge they turned partially over, thus exposing the floor to the wind. The wind, being obstructed by the wall of the house resting on the ground, became immediately compressed by the pressure from behind. The floor, unable to stand this pressure, went upwards, and the whole interior of the building became subjected to a strong pressure due to the compression of the air. This pressure was more than balanced on the side struck by the wind. On the other sides there was nothing to resist it, and the house was accordingly burst asunder.

We have therefore ample proof of explosion by compression, but none whatever of explosion by rarefaction.

The temperature at the centre is easily calculated. We have assumed, from the data, an average temperature of 76° on the surface of the ground, and a relative humidity of 65 per cent. This gives a dew point of 63°, or thereabout. This sudden decrease of 13°, together with the wet, is sufficient to account for the coolness experienced.

The lowering of the temperature at the centre of the tornado is due to the work performed by the air in expanding under diminishing pressure. The amount of this expansion, and consequently of the diminution of pressure, is easily obtained from the equation $\frac{a+t}{a+t_1} = \left(\frac{v_1}{v}\right)^{k-1}$ ("Zeuner's Grundzüge," p. 131), when a = coefficient of expansion = 491, t = initial temperature — 32, t_1 = final temperature — 32, v = initial volume, v_1 = final volume, and k = the relation between the thermal capacities of the air with constant volume, and with constant pressure = 1·41. The initial temperature in the case of the tornado = 76°, and the final temperature 63°. Consequently, $t = 76 - 32 = 44$, and $t_1 = 63 - 32 = 31$. Let us take $v = 1$. Then—

$$\frac{491+44}{491+31} = \left(\frac{v_1}{1}\right)^{1·41-1}, \text{ or } \frac{535}{522} = v_1^{·41}, \text{ or } v_1 = \left(\frac{535}{522}\right)^{\frac{1}{·41}} =$$
$$= 1·8219.$$

A cubic foot of air therefore expands, in passing from the circumference to the centre, to 1·8219 cubic feet. But the pressure is inversely as the volume. Therefore, since the

initial pressure was 28·5, $\frac{1 \times 28\cdot5}{1\cdot8219}$ = the pressure when the dew point is reached and the funnel commences = 15·69. This gives a diminution of 12·81 inches, corresponding to a pressure of about 6·40 lbs. per square inch, or 922 lbs. per square foot,—an amount of potential energy amply sufficient to account for all the phenomena.

The form and dimensions of the visible funnel did not therefore depend upon the diminution of pressure alone, but also upon the temperature and relative humidity of the inrushing winds. Its presence accordingly does not indicate a certain invariable velocity of the wind. When the air is moist, as it always is over the sea, a very small diminution of pressure, and consequently a comparatively light wind, would suffice to develop the phenomenon of a visible funnel. When, on the contrary, the air is very dry, winds of the utmost possible violence could be produced without a visible funnel. The funnel of dark cloud, therefore, by no means indicates the limits of the ascending current. This renders the explanation of the hovering up and down of the lower end of the funnel easy of comprehension. Varying humidity along the path of the tornado would be sufficient to produce this phenomenon.

As the air ascends the pressure diminishes, and the funnel consequently increases in width. The diminution of pressure perpendicularly is symmetrical, and the result is the symmetry of the form of the funnel.

The Sound.—Its loudness was extreme, yet of such a nature as to drown all other sounds without stunning. A man could stand by his house, as it was shivered to pieces, and not hear the noise of its breaking.

B. Whitaker heard it, together with others, at the distance of 60 miles. He says that even at that distance it indicated great force. The barometric gradient inclined from Mr. Whitaker to the tornado; consequently the propagation of the sound received no help from the wind. Thunder has very seldom been heard at a greater distance than 10 miles. The sound of the tornado was therefore thirty-six times louder than the loudest thunder. Heavy cannonading has been heard, it is said, at the distance of 90 miles, and "the report of a volcano at St. Vincent was heard at Demerara, 300 miles off." The voice of the tornado is, therefore, one of the strongest known to art or nature.

The Tornado in Illinois.—The general appearance of the tornado, as a whole, in Illinois, was somewhat different from that which it presented in Iowa. The ascending column of

air was no longer on one side of the centrifugal cloud, but apparently near its centre. Hence the witnesses generally described the cloud as it approached, as consisting of a dark cloud to the north-west, a less dark to the south-west, and a clearer space between. In Iowa all circumstances had combined in order to render precise observation easy. In Illinois the very reverse was the case. I shall therefore treat this portion of the subject in a few general remarks.

The tornado developed energy in this State quite as great as that displayed in Iowa. Its direction of revolution was the same. The disposition of the ruins were precisely similar. It showed the same and even a greater partiality for water-courses, turning abruptly from its course in order to follow them. Its direction varied between north-east and south-east, and was in the main easterly. At the moment when its velocity diminished, and before reaching Spoon River, it passed down close to the whirlwind, and even on its very path a perfect cataract of water. The height of the cloud must have been very great, for it was seen in the west for hours previous to its advent. There was less hail, and the sound was not so generally noticed as in Iowa.

The Identity of the Tornado in Iowa with that in Illinois.—When lost track of, the Iowa tornado was close upon the Iowa river, and travelling to the south-east. Its general course had, however, been easterly. A very severe rain-storm passed over the district between its point of disappearance and the Mississippi River. Trains were delayed by it and bridges washed away. On the Mississippi itself some damage was done to shipping by a violent gust of wind, which accompanied the rain. At Monmouth there was a severe thunderstorm. Twenty miles to the south of it the tornado again struck the ground. It was at Youngstown, at 5.45 P.M., or thereabout. From A. McKee's house, north of Washington, Iowa, to Youngstown, is a distance of about 63 miles in a straight line. The tornado was at McKee's house at 3.10 P.M. The difference between 5.45 and 3.10 is 2.35. The discrepancy due to longitude is four minutes, very nearly. The time, therefore, consumed by the storm in travelling from opposite Washington, Iowa, to Youngstown, Illinois, was two hours thirty-one minutes, or two and a-half hours. $63 \div 2\frac{1}{2} = 25\frac{1}{5}$, which is, therefore, the velocity of the storm, in miles per hour, in a straight line. The length of the journey was materially increased by the crookedness of the path. This brings the velocity nearly up to that with which the meteor traversed Keokuk and Washington Counties, Iowa. These facts, taken in

connection, irresistibly lead me to the conclusion that the tornadoes in the two States are one and the same.

The point where the tornado first struck the ground on South Skunk River is situated in 92° 20′ West Longitude and 41° 15′ North Latitude. It was finally lost track of near Peoria, in 89° 37′ West Longitude, and 40° 40′ North Latitude. The whole motion of the tornado, therefore, was 2° 43′ east, and 35′ south, or 143 miles east, and 41 miles south. The mean direction of its path was 24° 51′ south, and 65° 9′ east, and its whole motion 149 miles. The crookedness of its path renders its actual motion considerably greater.

In a late communication Mr. B. Whitaker, correspondent of the Smithsonian Institute of Warsaw and Illinois, states that the elevation of the summit of the cloud was about 18°.

The author thanks Elias Colbert, of the Chicago "Tribune," and the editors of the local newspapers at Washington and Sigourney, Iowa, for assistance rendered, and especially for publishing an extensive list of questions concerning the tornado, which secured for him valuable information from persons residing at a distance.

NOTICES OF BOOKS.

An Elementary Treatise on Steam. By JOHN PERRY, B.E. London: Macmillan and Co. 1874.

THIS is unquestionably a very useful little text-book, but the author has not altogether done himself full justice in its publication. In the first place the title is ill chosen, as it fails to convey any adequate idea of the real nature of the work. There is one good feature in this little volume which is not generally to be found in publications of a similar character, and this consists in a piecemeal treatment of the subject under consideration; after which examples, or problems, are given, to be worked out by the student in each branch, thus enabling him to ascertain for certain that he has fully comprehended the rules laid down in each case. Mr. Perry acknowledges having received much assistance, in the compilation of this little book, from the treatises of well-known authors in engineering science; but whilst he remarks—not perhaps without reason—on the absence of "experimental and other verification" of many of Prof. Rankine's calculations, there is nothing to show that he has himself attempted any practical verification of them.

This work is divided under four headings, viz.—I. Heat, &c. II. Steam-Engines and Boilers. III. Locomotives. IV. Marine Engines. From the range of subjects thus attempted within the compass of one small volume, it will at once be seen that they can each only be briefly touched upon; and this will be more apparent when it is stated that the important subjects of "Combustion" and "Calorific Power" of Fuel together occupy less than seven pages. In the chapters relating to different kinds of engines, the oft-told tales of their early inventions are again repeated, thus adding to the bulk of the volume without much enhancing its value as a scientific work. In the chapter relating to locomotives about eight pages are given to the subject of "Permanent Way, &c.," which had better have been left out altogether, as it is perfectly impossible to treat properly so important a subject thus briefly. The information regarding "Compound Engines" is scattered over six pages, in different parts of the book. Now this is a subject which is second to none other, when bearing on the question of the economical working of steam-engines, and might well have filled several pages, since to its introduction is due much of the saving of fuel effected in recent steam-engine improvements.

We make these remarks with no wish to injure the reputation of this work, but merely in order to point out where it may be improved should it reach a second edition. On the whole it

appears to have been carefully compiled, and is furnished with a useful Appendix of Tables and a good Index; and it will doubtless be found a valuable hand-book for students, to whom we can commend it with confidence.

Our Ironclads and Merchant Ships. By Rear-Admiral E. Gardiner Fishbourne, C.B. London: E. & F. N. Spon. 1874.

This work is evidently intended as a reply to the principles of Naval Construction as laid down in "Naval Science." The discussions and differences of opinion, which disclose themselves from time to time in our daily and scientific journals, regarding the modern type of war vessels, clearly show that there are two sides to the question as to whether those recently constructed have been built upon proper scientific principles. Now we know that the designs of Mr. Froude and Mr. Reed have long prevailed at the Admiralty, and the Schools of Naval Architecture disseminate the rules laid down by these authorities. Against the principles thus taught Admiral Fishbourne brings forward strong arguments, based upon scientific reasonings, and until his propositions are overthrown by still weightier arguments we shall continue—as we always have done—to have but little faith in the architects of a vessel that will flounder, as did the *Captain*, or that require "so much ballast to make them less dangerous," as the *Sultan* and *Invincible*. In consequence of Mr. Froude's propositions, deep empty spaces in the bottoms of ships were introduced by Mr. Reed, "expressly to facilitate the raising the engines, boilers, and other weights, because it has been ascertained that the tendency of ships to roll has been reduced by these means;" but, as is clearly shown by Admiral Fishbourne, this caused the errors of calculation to be on the *unsafe* side,—that of decreasing the stability below the quantity assigned by it. This principle of construction appears to us to be totally opposed to all previously accepted means of ensuring stability, as is practised, for instance, in the construction of a life-boat, where the spaces are arranged on the sides and ends of the vessel. To construct such deep hollow spaces at the bottom of the vessel must tend to give that part of it increased buoyancy, which, as we have already shown, has to be overcome by a heavy amount of ballast, as would be the case if a boy covered the bottom of his little yacht with cork. The plan adopted of calculating a ship's stability upon the metacentric method in a still basin is justly condemned by our author as not enabling a safe estimate to be made, "because a vessel, when in motion ahead, possesses a greater total stability than her inclination indicates, for it is necessary to provide for sufficient stability when a ship is without motion ahead, and, therefore, without hydrodynamic

stability," and he carries his argument out fully to prove that the present metacentric method of calculation is erroneous. The question of buoyancy is next dealt with in a manner which leaves Mr. Reed but little cause to be satisfied with his own performances. In our ironclads great buoyancy is given at the bottom of the vessel, whilst the heavy armour plating on the sides gives weight where buoyancy should exist, for it is at this very point where the life-belt of the ship should be placed. But Admiral Fishbourne is not content with merely stating his arguments at length, but they are supported at length by mathematical calculations which leave no grounds for concluding otherwise than that his theory is based upon true scientific principle.

Practical Solid or Descriptive Geometry. By W. TIMBRELL PIERCE, Architect, late Lecturer on Geometrical Drawing at King's College, London (*vice* Prof. Bradley), and Harrow School. London: Longmans, Green, and Co. 1871.

THIS is a work which has doubtless often been needed by the student of practical solid Geometry. Although many excellent works of this character exist in foreign languages, we have none of a really useful character published in English. A thorough knowledge of the principles of descriptive geometry is needed by many professional men, to enable them to represent solid objects on a plane surface, and hence its application to all the arts of construction, such as architecture, engineering, fortification, &c. To the artist, also, a knowledge of this subject is most important, as enabling him to draw in true perspective, cast the shadows of objects truly, and obtain the true position of the *locus* of greatest light on an object.

The first part of this work treats on orthographic projection, and is divided into six chapters as follows:—I. Definitions, &c.; Projections of Points and Lines. II. On Straight Lines and Planes. III. The Five Regular Solids, viz., the Tetrahedron, the Cube, the Octohedron, the Dodecahedron, and the Icosahedron. IV. Consists of Examples and Problems illustrating the principles of construction contained in the preceding chapters. V. On Section Planes and the Intersections of Solids. VI. On Tangent Planes and Surfaces in Contact.

The second part of the book is on Perspective, or Radial Projection, which is the application of the principles, enunciated in the first part, to practice on a plane surface: their application to the several arts of construction is a further development of the subject, which Mr. Pierce promises to explain in a future work. The careful manner in which the student is gradually advanced from one point to another, through the pages of this book, must render it one of great value to the student as a text-book, whilst

the manner in which it has been got up leaves nothing to be desired: it is printed in a good bold type, and illustrated with thirty-seven plates carefully drawn and clearly lettered.

Results of an Experimental Enquiry into the Mechanical Properties of Steel of Different Degrees of Hardness, and under Various Conditions, Manufactured by Charles Aspelin, Esq., Westanfors and Fagersta Works, Sweden. By David Kirkaldy. London: published by the Author. 1873.

Within the last few years a revolution has been gradually effected in constructive art by the introduction of a material which combines, in an eminent degree, strength, hardness, and toughness, and which admits, according to its mode of preparation, of great variations in its physical properties, and is therefore applicable to a vast variety of purposes. As the applications of steel are constantly being extended, it becomes a matter of great importance to the mechanical, civil, and mining engineer that the physical properties of the material should be accurately determined. Few men are better qualified to conduct such enquiries than is Mr. Kirkaldy. His great experience in testing the mechanical properties of metals is the best guarantee that his tests are judiciously applied, and the results intelligently interpreted.

In the present work Mr. Kirkaldy gives the results of an elaborate enquiry into the physical characters of a large number of samples of Swedish steel, made at Fagersta. Some of these tested specimens were sent to the Paris Exhibition of 1867, but the greater number were prepared for the late Vienna Exhibition. The results exhibit the behaviour of the metal when subjected to pulling, thrusting, bending, twisting, and shearing strains; in fact, when tortured in every way that can possibly be of interest to the engineer. These results are exhibited in a series of carefully-arranged tables, which, although of great value for reference, are not of course attractive to the reader. They deserve, however, to be carefully studied by all who are interested in the manufacture and applications of modern steel.

Annual Record of Science and Industry for 1873. Edited by Spencer F. Baird. New York: Harper Brothers.

This work, which has now reached its third year, is in its general objects similar to the "Year-Book of Facts." It has, however, certain unmistakable advantages over its English contemporary, Its information is not drawn from ordinary newspapers.

The scientific and technological journals of France, Germany, Holland, Switzerland, Italy, and Denmark, as well as of America and England, have been carefully laid under contribution. Hence paragraphs found in this volume may be quoted or appealed to with a reasonable amount of confidence. Another useful feature is, that not merely the names of the journals quoted are given, but the date and the page, so that it is easy to verify any citation, or, in case of on abstract, to find full particulars. That such an arrangement much enhances the value of any work of this class is obvious. The classification is also natural, or scientific, which is the same thing.

To an Englishman, perhaps the most interesting feature of the book will be found in the notices of various American institutions for the promotion of science. In this respect we cannot help expressing the fear that America is evincing greater energy, greater liberality, than ourselves. Of this we find many instances. The museum at Yale College has secured the finest specimen of the pterodactyl ever discovered. The British Museum was negotiating for the purchase, but our rivals over the Atlantic telegraphed to the owner to name his price, and paid it. What have we in England equivalent to the Stevens Institute of Technology at Hoboken, founded, it must be remembered, by the munificence of a private individual? How few instances do we hear in England of bequests or donations to museums, free public libraries, and institutions for training up discoverers and inventors! The liberality of our wealthy men seems to take every channel rather than this. We do not at all think that in natural aptitude for the successful study of Science the British and Irish nations are inferior to any people on the globe. But supremacy in this respect, just as in war, can no longer be attained by the spontaneous and desultory efforts of individuals. General, systematic, persistent action is wanted. Original research has to be conducted on a changed ground, and can no longer, as in the days of Scheele, or even of Davy, be effected with a few phials, tobacco-pipes, gallipots, and saucers. The influences which Decandolle finds most powerful in "advancing science, by increasing the number of those who prosecute it in the right spirit, are, *first*, a well-organised system of instruction, *independent of parties*, tending to awaken research and to assist young persons devoting themselves to science; *second*, abundant and well-organised material means for scientific work, libraries, observatories, laboratories, collections, &c.; *third*, freedom of utterance, and publication of any opinion on scientific subjects without grave inconvenience." To what extent are these influences at work among us? We have museums; some of them placed where they are accessible to few but fashionable idlers; others carefully closed in the evening, on public holidays, and, in fact, at every time save what are called "hours of business." We have libraries open to the same charges, whilst

the formation of establishments of this kind under Mr, Ewart's Act has been opposed—and in too many instances successfully—by the interests now dominant in the House of Commons. Our "system of instruction," if "organised" at all, seems arranged rather for the aggrandisement of parties and interests than for training up thinkers and discoverers. Did not Lord Sandon, in his place in Parliament, lately oppose a project for extending the curriculum of elementary education, by asserting that the purpose of the movement was to "crowd out" theological teaching? Instead of regaining the ground we have lost in competition with foreign nations, it may be gravely questioned whether we are not falling further and further into the rear. This is a melancholy prospect for those who are aware of what is going on, and who find their words of warning drowned amidst the uproar of the "interests."

Legal Responsibility in Old Age. By G. M. Beard, A.M., M.D., New York: Russell.

The title of this thoughtful, interesting work is far from conveying a full idea of its nature and contents. There is a very common notion that the mind attains its maximum development long after the maturity of the body; often, even, after the vigour of the latter is plainly on the decline. Hence wisdom is regarded as no less a normal attribute of old age than is strength of youth. This view the author has subjected to a careful criticism, and, as the conclusion of his researches, has pronounced it unwarranted by facts. His method—the only one applicable in the case—has been to prepare "a list embracing nearly all of the greatest names of history whose lives are recorded in sufficient detail to be of value in such an investigation," and to note "the age at which they did the original work by which they have gained their fame." From a comparison of these data he has deduced "the period, the decade, and the year of maximum productiveness, and the various grades between this and the period, the decade, and the years of the least productiveness." The enquiry has been extended to great men of all departments, scientific, artistic, and practical.

As a general result, the author finds that 70 per cent of the work of the world is done before the age of 45, and 80 per cent before that of 50. Going into more exact detail, Dr. Beard divides the active portion of human life into six decades:—

The golden	decade	is between	30 and 40.
The silver	,,	,,	40 and 50.
The brazen	,,	,,	20 and 30.
The iron	,,	,,	50 and 60.
The tin	,,	,,	60 and 70.
The wooden	,,	,,	70 and 80.

"The golden decade alone represents nearly one-third of the work of the world, and nearly 25 per cent more dates than the silver. The best period of fifteen years is between 30 and 45. The advantage of the brazen over the iron decade is very striking, and will cause surprise. There is considerably more work done between 35 and 40 than between 40 and 45." Dr. Beard, we may observe, is not particularly happy in the selection of metals to illustrate the comparative value of the respective decades of life. Tin, for instance, may reach fifty times the value of iron.

It may be urged that many men of genius die young, and that, possibly, if not thus prematurely cut off, they might have continued to distinguish themselves up to an advanced time of life. This objection the author anticipates by remarking that "the average age of the great personages from whose lives the law is derived is not far from 66 years." He continues—"On the average the last *twenty* years in the lives of original geniuses are unproductive." "The broad fact, then, to which these statistics lead us is that the brain follows the same line of growth, maturity, and decay as the rest of the body; that the nervous, muscular, and osseous systems rise, remain, and fall together, and that the received opinion that the mind—of which the brain is the organ—developes and matures later than the power of motion, or of physical labour and endurance, is not sustained by the facts of history."

Having thus stated his general conclusion, the author takes into consideration qualifying circumstances and exceptions. He points out the distinction between *original* work, requiring enthusiasm, and *routine* work, depending on experience. The former he considers the especial province of the young, and the latter of the old. "The people unconsciously recognise this distinction between the work that demands enthusiasm and that which demands experience, for they prefer old doctors and lawyers, while in the clerical profession—where success depends on the ability to constantly originate and express thought—young men are the more popular, and old men, even of great ability, are shelved or neglected." Here we may remark that in England, and still more on the Continent, the clerical profession is far from being the one from which originality of thought is most urgently demanded. The faculty of expression often seems to survive the power of origination. Thus the "old man eloquent" may often, in the evening of his days, embody in words and give to the world the ideas which he had thought out in the days of his prime. "The most marked exceptions to the law are found in the realm of imagination; some of the greatest poets, painters, and sculptors have done a part of their very best work in advanced life." We might here, however, ask—whether enthusiasm is not at least as necessary in the elaboration of an epos, a drama, or a painting, as in that of a scientific theory or a patent?

The following passage is of profound significance :—" Children born of parents one or both of whom are between 25 and 40 are, on the average, stronger and smarter than those born of parents one or both of whom are much younger or older than this." Political economists, we believe, claim the privilege of being ignorant of every science except their own, and in virtue of that ignorance they counsel late marriages, in the hope of checking the too rapid increase of population. Unfortunately their scheme reduces not merely the number, but the stamina, physical and mental, of a nation.

Dr. Beard next examines the moral faculties, and finds them subject to the same law. " It does not follow," says he, " that when a man declines in moral principle that he necessarily becomes a horse-thief—a loss of active moral enthusiasm is frequently all that is noticed." Among the causes of moral decline in old age the author enumerates—" Over-exercise through life of the lower at the expense of the higher nature ; disease of the brain, or of other parts of the body that react upon the brain, and intellectual decline." " Death," he remarks, " is more frequently a process than an event. When conscience is gone the constitution may soon follow." In illustration of the decline of the moral faculties in old age, we are referred to the lives of a number of historical characters, among whom figure the names of Carlyle and Ruskin. A note on the death of Agassiz must not be overlooked :—" To his friends it is well known that Agassiz began to die several years ago. His death can be hardly regarded as a loss to scientific research, for he had long ceased to be productive. So far as he lives in the future of science, it will be mainly for the original work that he did before he reached his fortieth year. The intemperate manner of his opposition to the theory of evolution, by which he was so rapidly winning favour among the thoughtless and ignorant, and so rapidly losing favour among the conscientious and scholarly, may find its partial—if not complete—explanation in the exhausted condition of his brain."

We are here reminded of those changes of opinion on controverted points—theological, political, or philosophical—which eminent men sometimes experience in the decline of life. The old friends of such a character seldom fail to denounce him as a pervert and a renegade, and to accuse him of corrupt motives. Those whom he has just joined hail him a convert, and rejoice that his eyes have at last been opened to the truth. Both are wrong. Such changes are often sincere, but they spring not from illumination, but from a darkening of mental vision. The accession of a man, once great, who has fallen into second childhood, is no great triumph to any party. What matters it if, in his declining years, Liebig showed leanings to Obscurantism? One of the most ordinary phases of mental decay is the mingled incapacity and unwillingness to recognise new discoveries or new

ideas. We have seen it somewhere stated that no physician who had reached the age of 50, at the time when Harvey made public his discovery of the circulation of the blood, could ever be induced to give it his recognition. Dr. Oliver Wendell Holmes, one of the most original thinkers that America has produced, says—"New ideas build their nests in young brains; revolutions are not made by men in spectacles, as I have heard it remarked; and the whisperings of new truths are not caught by those who begin to feel the need of an ear-trumpet."

We have thus far expounded Dr. Beard's opinions, and we fully admit their value. The evidence he has brought forward to prove that early life is the season for original work scarcely admits of doubt; but we still question whether the development and the decline of the various faculties of the human system are so strictly synchronous as he maintains. We have known a man of powerful frame and active habits, who, in the sixties, had become quite childish, and "babbled o' green fields" in a style very unedifying; yet his pedestrian powers were the envy of many men on the sunny side of 40. Conversely, our readers will doubtless recall cases of men, physically feeble, but yet retaining their memory and their reasoning powers, little, if at all, impaired. It seems to us that if any faculty is naturally strong, and has been judiciously cultivated, it will preserve its vigour after the system in other respects shows manifest symptoms of decay.

Into the application of the doctrines above explained to forensic medicine and to jurisprudence we do not propose to enter. The author raises the questions—"To what extent is the average responsibility of men impaired by the change that the mental faculties undergo in old age?" And—"How shall the effects of age on the mental faculties be best brought to the attention of our courts of justice?" That these questions will have to be taken into account by the legislation of the future is undeniable.

The Year-Book of Facts in Science and Art. By JOHN TIMBS. London: Lockwood and Co.

THIS annual still pursues the even tenour of its way, giving a yearly summary of discoveries, inventions, and what are supposed to be the most important facts connected with science and industrial art. The utility of such a work is indisputable, and that it meets a recognised want may be inferred from the fact that it has regularly appeared for a quarter of a century. We cannot, however, but regret that much matter is inserted on authority which the scientific public can scarcely recognise. The origin of many of the paragraphs and extracts is not stated, whilst very many others are drawn from ordinary newspapers, English and American. Now the inaccuracy of the scientific

information given in such journals, from the "Times" downwards, is a matter of notoriety. Indeed, strictly scientific papers, English or foreign, appear to have been eschewed by the editor. In the section on Chemistry, which extends to no more than eleven pages, we find extracts from the "Times" and the "Illustrated London News," but not a passage from Liebig's "Annalen," from the "Berichte" of the German Chemical Society, or the "Chemical News." A similar complaint might be made concerning the chapters on Zoology and Botany, where among the authorities figure the "Homeward Mail, the "Globe," the "Pall Mall Gazette," and the "Grocer."

As regards the classification of the matter, the editor appears to proceed on principles of his own. Thus under the head "Chemical Science," we find paragraphs on "Sunlight for the Sick," on "Prompt Remedies for Accidents and Poisons," and on "Opium Smoking in New York!" On the other hand, "Butter and its Adulterations" and "Tea and its Adulterations" rank under "Mechanical and Useful Arts." On the other hand, we find an article on "Compressed Peat"—abridged from the "Times"—in the chapter on Geology and Mineralogy.

But the gravest complaint against this compilation is the very small amount of light which it throws upon the progress of the sciences, and upon their practical and industrial applications. An intelligent man, without any especial acquaintance with chemistry, would, from merely reading Mr. Timbs's "Year-Book," be utterly unable to form any adequate idea of the part which that science is now playing in the development of civilisation. We make these remarks not in a captious or "hyper-critical" spirit, but in the hope that the defects we point out may be in future amended, and that the work may be made—as it easily could—useful to the pubic, and a credit to our technological literature.

The Treasury of Natural History; or, a Popular Dictionary of Zoology. By SAMUEL MAUNDER. Revised and Corrected, with an Extra Supplement. By E. W. H. HOLDSWORTH. London: Longmans, Green, and Co.

THIS compact volume, as far as its size allows, gives a clear and generally correct account of the orders, families, and genera, as well as of the more important species of the animal world. The illustrations are numerous. We do not agree with the description given of the common viper. The ground colour of the male is said to be a dirty yellow; that of the female deeper. Among the numbers we have seen the males were of a pale ash-grey, and the females of a decided copper colour. We cannot help expressing our dislike of the very minute type which has

been selected. Surely economy in cost and bulk is purchased too dearly at the risk of impaired eyesight. The syllabus of Practical Taxidermy will be found very useful to incipient naturalists.

United States Commission on Fish and Fisheries. Commissioners' Report, 1871-1872. *The Fisheries of the South Coast of New England.* Washington: Government Printing Office.

This is a most elaborate "Blue Book," extending to 850 pages, and abundantly illustrated. The condition of American fisheries, the arguments in favour of their regulation by law, the reports of state commissions and of European authorities on this subject are given with great minuteness. We find also a description of the various kinds of apparatus used in capturing fish on the sea-coast and lakes of the United States, and of patents granted to the end of 1872 for inventions relative to the capture, utilisation, or cultivation of fish and marine animals.

The greater part of the volume, however,—and this is a feature to which we desire to call special attention,—is taken up with matter specially interesting to the naturalist. There is a report of Dr. Farlow on the sea-weeds of the south coast of New England, and one by Mr. A. E. Verrill on the invertebrate animals of Vineyard Sound and the adjacent waters, with an account of the physical characters of the region. We must call particular attention to the list of species found in the stomach of fishes, as throwing a valuable light on their food, and to a paper on the metamorphoses of the lobster and other crustaceans. Mr. Theodore Gill's catalogue of the fishes found on the east coast of North America, and Mr. S. F. Baird's list of fishes collected at Wood's Hole Harbour, with the table of temperatures for the year 1873, also furnish valuable data for the student of marine zoology. Very curious are the quaint reports on the fauna, terrestrial as well as aquatic, of New England and the other Atlantic Districts in the old colonial days. A "squiril" is described as "red, and he haunts our houses, and will rob us of our Corne, but the Catt many times payes him the price of his presumption." Here is a hint which we commend to the professors of cookery at South Kensington:—"We used to qualifie a pickled *Herrin* by boiling of him in milk." Scientific controversy was, in those days, carried on with some disregard of courtesy. "One writes that the fat in the bone of a Basse's head is is braines, which is a lye."

The whole volume is highly creditable to its compilers as well as to the Government which has ordered its publication.

PROGRESS IN SCIENCE.

MINING.

In consequence of the great development of the manufacture of spiegeleisen, both in this country and on the Continent, the spathic iron ores, or native carbonates of iron, which are so largely used in this manufacture, have been brought, within the last few years, very prominently into notice. A paper on the distribution of these ores was read by Mr. C. Smith at the recent meeting of the Iron and Steel Institute. In this country there are numerous deposits of spathose ores, the most important perhaps being those of the Brendon Hills, which are worked by the Ebbw Vale Company. Here the ores occur in irregular veins, coursing through Devonian clay-slates, and recalling the conditions under which similar ores occur in Rhenish Prussia. The spathic ores of the Rhine Provinces are distributed in two separate districts—the one near Coblenz, and the other to the east of Cologne; the latter being by far the larger area, and containing the well-known Stahlberg. In Styria immense deposits of spathic ores have long been worked, especially at the Eisenerz, for the manufacture of charcoal-iron. Sweden seems to be destitute of these ores, the magnificent iron-making resources of that country consisting mainly of magnetites and red hæmatites. The mineral called Knebelite—a silicate of iron and manganese—is worked in Sweden for use in the spiegeleisen manufacture.

Some deposits of tin-ore, which promise to become of enormous value, have been discovered at Mount Bischoff, in Tasmania. We believe that Mr. Gould, the Government geologist, who has recently returned from Tasmania, bringing with him some fine examples of these ores, will shortly communicate a description of the deposits to the Geological Society. Some of the tin-stuff is associated with much ochreous oxide of iron. In addition to the lodes, there are said to be alluvial deposits of vast magnitude. Mr. W. Ritchie speaks of one bed of wash-dirt, 37 feet in thickness, rich in tin from the surface to the bottom of the shaft.

A rapid tour through Gippsland, made in the early part of this year, has enabled Mr. R. Brough Smyth to contribute to a recently-issued official Report some remarks on the mineral resources of this part of the Colony of Victoria. Many parts of the country are said to be rich in gold, but though some few workings—such as the Walhalla mines—have been for some years in operation, the gold-fields of Gippsland have yet to be developed.

From Persia we hear of recent discoveries of several beds of coal, and of deposits of nickel ores, by Dr. Tietze, of Vienna, who is now engaged in exploring this country with special reference to its minerals. The coal is said to be of Mesozoic or Secondary age. Much of the coal of Queensland, and valuable deposits of mineral fuel elsewhere, are also referable to the Mesozoic period.

With reference to the age of some of the American coals and lignites, Dr. J. S. Newberry has recently communicated to "Silliman's Journal" a paper in which he takes a general review of the occurrence of coal in the Far West, and seeks to show that the deposits of lignite and the various plant-beds of the Western States are mostly of Cretaceous age, whilst some belong to the miocene period. The coal of Vancouver's Island is said to be Cretaceous.

A deposit of bismuth ore has been discovered near Meymac, in the Department of the Corrèze, in Central France. The ore occurs in a quartzose vein running through granite, and is accompanied by wolfram, mispickel, and pyrites. The bismuth exists chiefly as the native metal, but partly as sulphide and as oxide.

The first volume of the "Transactions of the American Institute of Mining Engineers" has been recently published in Philadelphia. This volume contains a rich collection of papers on mining and metallurgy, selected from communications made to the Institute since its foundation in 1871.

METALLURGY.

No one will be disposed to question the good sense of the governing body of the Iron and Steel Institute in awarding the first Bessemer medal to Mr. I. Lowthian Bell, as a fit recognition of his services to the science and practice of iron-smelting.

In delivering the Presidential Address at the recent meeting of the Institute, Mr. Bell referred to the advance of mechanical puddling in this country, at the same time expressing his fear that at present the success of Danks's system had hardly been found equal to what might have been expected from the Report of the Commissioners appointed by the Institute to examine into the working of this system in America. Danks's furnaces have now been erected by Messrs. Bolchow and Vaughan, Messrs. Hopkins, Gilkes, and Co., the Erimus Iron Company, and the North of England Industrial Iron Company, whilst in North Staffordshire they have been introduced by Mr. R. Heath. It is therefore to be expected that we may soon have opportunity of thoroughly testing the practical value of this system of puddling. Mr. J. A. Jones, who has had perhaps more experience than any one else on this subject, contends that the Danks principle is perfectly sound, and merely requires development, and perhaps a little mechanical modification, to make it commercially a success. He maintains, indeed, that a few months hence we shall be able to turn out better bars, cheaper by the rotatory furnace than by the old method of manipulation. It has been said that the Danks furnace is, from a mechanical point of view, a complete failure, but the details of construction may be easily modified without affecting the general principle. At the Erimus Works a furnace is to be erected which will combine some of the principles of the Danks with those of the Crompton furnace. Mr. Crompton's furnace, in which finely-divided fuel is introduced with the blast, has been working with great success at Woolwich.

A mechanical puddling-furnace of peculiar construction, devised by M. Pernot, has been worked by MM. Petin and Gaudet, at St. Charmond. The puddler is an iron bowl, mounted on a cast-iron carriage, which is caused to rotate by machinery. The bloom may be divided into as many portions as may be desired, instead of being limited to a single large ball. The iron is said to be of high quality, while the mechanical arrangements admit of being applied to existing plant.

Spiegeleisen formed the subject of an interesting paper communicated to the Iron and Steel Institute by Mr. G. J. Snelus. This paper was intended to supplement Mr. Forbes's valuable "Report on the Manufacture of Spiegeleisen on the Continent," published some time ago in the Journal of the Institute. The use of spiegeleisen, originally suggested by Mr. Mushet, was worked out by Mr. Bessemer, and its manufacture has now become an important branch of manufacture even in this country. Instead of relying for our supply, as formerly, upon the Continent, we have found ores suitable for its production, and have established its manufacture at Ebbw Vale, at the Landore Steel Works, at Dowlais, at Middlesbro', at Sheffield, and in Cumberland.

Attention has been recently called to the part which silicon plays in pig-iron during its conversion in the Bessemer process. By oxidation in the converter, the silicon is converted into silica, which passes into the slag, and during this oxidation a much greater quantity of heat is evolved than could be obtained by the oxidation of an equal weight of carbon. According to the experiments of Troost and Hautefeuille, the carbon of cast-iron, at a temperature above that of the fusion of the metal, reduces silica, the silicon replacing the carbon in the pig.

Some experiments have been made by Mr. C. H. Morton on the condition in which silicon exists in pig-iron. The experiments were made on a No. 1 Bessemer pig, containing 4·612 per cent of silicon. They tended to show that the silicon does not exist in a free state mechanically associated with the iron, but rather in a combined form as a definite silicide of iron, just as carbon may exist as a carbide; the silicon does not appear, however, to assume a graphitoidal form in the iron, corresponding with the uncombined carbon of cast-iron.

In recently describing a mass of meteoric iron from Howard Co., Indiana, Dr. J. Lawrence Smith takes occasion to make some general remarks on iron, which are not without interest to the metallurgist. The Indiana iron appears to be a true meteorite; it contains 12·29 per cent of nickel, 0·65 of cobalt, and 0·02 of phosphorus; yet when a polished face is acted on by acids it does not exhibit the well-known Widmannstättian figures. Some authorities have maintained that these markings result from the accumulation of an alloy richer in nickel than the mass of the iron; whilst others have referred them to a definite phosphide of iron and nickel, accumulated along certain lines of crystallisation. Neither of these theories, however, satisfactorily accounts for their presence in some irons and their absence from others. Dr. Smith believes that in the solidification and crystallisation of iron there is a tendency to eliminate the foreign constituents to the exterior of the crystals. If, then, a meteoric mass has consolidated rapidly, the phosphorus might be so diffused as to afford no marked indication of its presence, but if consolidated slowly there would be a more or less perfect elimination of the phosphorus in parts representing spaces between the crystals of the mass. We may remark, in passing, that Dr. Smith has found that the presence of 1 per cent of phosphorus, or less, in cast-iron, enables the metal to resist the action of concentrated sulphuric acid to a greater degree than when the metal is entirely free from phosphorus.

Natro-metallurgy is a term applied to a process for refining impure lead, recently introduced by MM. Payen and Roux, of Marseilles. The foreign metals present in the hard lead are removed by means of molten caustic soda, the oxidation being facilitated by the introduction of jets of steam, a blast of air, or addition of nitrate of soda.

Some notes on the metallurgy of bismuth have been communicated to the "Annales de Chimie" by M. A. Valenciennes, who describes the process which he has introduced at St. Denis for working the Bolivian ores. Having been first roasted in a reverberatory furnace, the ore is mixed with 3 per cent of charcoal and a flux composed of lime, soda, and fluor-spar. After reduction the products separate in order of density—at bottom a button of bismuth, above this a regulus of sulphides of bismuth and copper, and on the top a vitreous slag containing silicate of iron. The crude bismuth contains about 2 per cent of antimony and lead, and about the same proportion of copper, with traces of silver. If used for preparation of subnitrate, the bismuth is fused with nitre, to oxidise the antimony, whilst the other foreign metals are separated by the wet way. The regulus may be roasted, and subjected to similar treatment as the original ore. In treating the French bismuth-ores recently discovered at Meymac recourse is had to a wet process. The finely-divided ore is digested in hydrochloric acid, the solution neutralised, and water added, whereby a subchloride is precipitated, and this precipitate is then reduced by contact with metallic iron. The bismuth thus obtained is dried, and fused with an alkaline flux. A similar mode of treating the French ores has been carried out by M. Carnot.

Dr. Laspeyres, of Aix-la-Chapelle, has contributed to the "Journal für Praktische Chemie" a description of some artificial crystals of metallic antimony, accidentally obtained in slags at the Münsterbusch Lead-Works, near Stolberg. Some antimonial residues and lead slags were smelted in a blast-furnace, with the view of obtaining a hard lead rich in antimony. The crystals appear at first sight to be regular cubes, resembling the well-known artificial galena, but are really rhombohedra of nearly 90°, combined with the basal

planes, and in some cases twinned so as to exhibit the characteristic re-entering angles.

MINERALOGY.

A mineralogical paper of considerable interest has been communicated to the American Academy of Arts and Sciences by Prof. J. P. Cooke, jun., in which he reviews the history of those micaceous minerals which may be grouped together under the general name of *Vermiculite*. This name was originally applied by Mr. T. H. Webb to a peculiar mineral which at the time of its discovery excited much interest by its curious behaviour when heated; the substance exfoliated prodigiously, and the little scales opened out in worm-like threads, which suggested the original name. This remarkable exfoliation and the apparent increase in volume are referred by Prof. Cooke to the loss of water of crystallisation, and may be compared with the well-known efflorescent phenomena presented by the dehydration of certain crystalline salts. Other minerals possessing similar pyrognostic characters have since been discovered, and a comparison of these minerals has led the author to extend the use of Vermiculite as a family name. Three distinct species are now included in this group: first, *Jefferisite*, a mineral discovered by Mr. W. Jefferis at West Chester, Pa., and described by Prof. Brush; secondly, *Hallite*, a new species founded by Prof. Cooke on specimens collected by Mr. J. Hall at East Nottingham, Chester Co., Pa.; and thirdly, *Culsageeite*, another new species discovered in Col. Jenks's Culsagee Mine in North Carolina, which mine has yielded the remarkable specimens of corundum noticed in our report last quarter. Prof. Cooke discusses in much detail the crystallographic, optical, and chemical characters of Jefferisite, Hallite, and Culsageeite, with special reference to their relation to the group of micas.

Our knowledge of the mineralogy of the Argentine Republic is much increased by a paper recently communicated to Tschermak's "Mineralogische Mittheilungen" by Dr. A. Stelzner, who was appointed a few years ago Professor of Mineralogy in the University of Cordoba. The Sierra of Cordoba, rising like an island from the surrounding pampas, is composed of three parallel ridges, which consist principally of crystalline slates associated with masses of granite in which are large quartz-stocks containing minerals of considerable interest. Although consisting chiefly of quartz, these stocks invariably contain mica, as well as large crystals of orthoclase-felspar. Beryl is also found in six-sided prisms of unusual size, whilst apatite and triplite occur subordinately. But the most interesting of this group is the rare mineral columbite, specimens of which have been analysed in Cordoba by Dr. Siewert. Another series of minerals described by Stelzner occurs in crystalline limestones, associated with the metamorphic rocks of the Sierra.

Prof. A. H. Church has communicated to the "Chemical News" his analysis of Ashantee gold. Those who are familar with the fine collection of nuggets and ornamental objects lately exhibited by Messrs. Garrard will remember that most of the specimens presented a rich colour, heightened by association with a red ferruginous earth, more or less adherent to the surface. Church's analysis gave—Gold, 90·055; silver, 9·940; iron and copper, traces.

The same chemist has also published in the "Chemical News" his analysis of some clean grain-gold from a burn at Wanlock-head in Dumfriesshire. A good deal of historical interest clings to the gold-fields of Southern Scotland, though well-nigh forgotten at the present day. Worked by Sir Bevis Bulmer in the sixteenth century, they yielded the gold from which the unicorns and the bonnet-pieces of James IV. and V. were coined. It was these mines, too, that were at one time to be worked by a company of twenty-four gentlemen to be created "Knights of the Gold Mines." At present, however, the Clydesdale washings yield only specimens for the cabinets of the curious. Prof. Church finds the Wanlock-head gold to contain—Gold, 86·60; silver, 12·39; iron, 0·35. A specimen of Sutherlandshire gold has yielded to Mr. G. H. Makins 79·22 per cent of gold and 20·78 per cent of silver. Pliny tells us that when the proportion of silver in gold exceds one-fifth, the substance is

called *electrum*. According, then, to this definition, which is still adhered to by some mineralogists, the Sutherland gold is an electrum.

Two specimens of what appeared to be native silver have been analysed by Prof. Church, and the results published in the "Chemical News." Both came from Allemont, in Dauphiny, and were formerly in Heuland's collection. One contained—Silver, 71·69; mercury, 26·15; antimony and traces of arsenic, 2·16. The other contained—Silver, 73·39; mercury, 18·34; antimony and arsenic, 8·27. These figures do not correspond precisely with analyses of any *native amalgams*, nor with the antimonial silvers called *dyscrasite*, the latter being always regarded as non-mercurial.

Although the Jubilee volume of "Poggendorff's Annalen," which has recently been issued in commemoration of the fiftieth year of its publication, is for the most part devoted to memoirs on the several branches of physical science, it is nevertheless of considerable interest to the mineralogist. Prof. Rammelsberg gives a masterly sketch of the history of the Chemistry of Minerals during the last half century: Vom Rath publishes some valuable crystallographic studies; and Hankel gives a *resumé* of his researches on the pyro-electricity of certain minerals. In the same volume Dr. T. Scheerer has a paper "On the Formation of the Minerals which accompany Ores." His investigations relate to the conditions under which the most common non-metallic minerals of lodes—calc-spar, quartz, barytes, and fluor-spar—have been produced. As these may be formed in the wet way, and have probably been so formed in nature, it is only rational to conclude that the ores which accompany them in mineral veins owe their genesis to similar causes.

Two kinds of pseudomorphs after rock-salt have been discovered in sinking shafts for working some deposits of salt at Westeregeln, near Stassfurt, in Prussia. In one case we have a curious example of rock-salt pseudomorphous after rock-salt, whose formation may be thus explained: chloride of sodium was originally crystallised in cubic forms in a matrix of soft clay; the crystals were then dissolved, leaving cubic cavities, which were afterwards more or less distorted by movement of the surrounding clay; the walls of the cavities were next lined by a thin coating of quartz-crystals; and, finally, the drusy cavities became occupied by a second deposit of salt, which thus assumes abnormal forms and presents the curious feature of cleavage not necessarily parallel to faces of the crystal, since the internal structure bears no relation to external form.

The other Westeregeln pseudomorphs are six-sided crystals, like those of carnallite, but composed almost exclusively of chloride of sodium. Both kinds may be found in the same hand-specimen.

Under the name of *Rhagite*, Prof. Weisbach has recently described a hydrous arsenate of bismuth, discovered among the new uranium ores at Schneeberg, in Saxony. The name was suggested by the botryoidal form and grape-green colour of the mineral.

A description of a new Mexican mineral has been communicated by Don Ant. del Castillo to the Journal "La Naturaleza." The mineral is said to be a double selenide of bismuth and zinc.

The name of *Huantajayite* has been bestowed upon a Peruvian mineral by M. Raymondi, of Lima. Its composition seems peculiar; it is said to be a double chloride of sodium and silver, containing 89 per cent of the former and 11 of the latter salt. It occurs in small colourless cubes, associated with the chloride and chloro-bromide of silver and oxychloride of copper. This occurrence would seem to show that the metallic chlorides and bromides in Peru may owe their origin to the action of sea-water on the minerals of the lodes.

Grochauite is the name which Dr. Websky proposes for a new species closely related to clinochlore, occurring in serpentine at Grochau in Silesia. It is associated with an ore of chromium described by Dr. Bock as *Magnochromite*, which appears to be a member of the spinel group, rich in magnesia.

Websky has recently analysed the mineral called *Strigovite*, which occurs in granite at Striegan in Silesia.

A mineral resembling chalcomorphite has been found in limestone enclosed in the lavas of Ettringen, near the Lake of Laach, and has been recently described by Herr J. Lehmann as *Ettringite*.

The extremely rare mineral *Osm-iridium*, an alloy of osmium and iridium, has been discovered in small quantity near Stockyard Creek, Gippsland, in the Colony of Victoria.

Prof. Rammelsberg has communicated to the German Geological Society, at Berlin, several new analyses of *Idocrase*, or *Vesuvian*, accompanied by a review of our knowledge of the chemical composition of this species.

Some notable examples of the occurrence of crystals of quartz have been collected by Dr. Websky, and described in a crystallographic paper published in Leonhard and Bronn's "Jahrbuch."

ENGINEERING—CIVIL AND MECHANICAL.

Guns.—A paper of some importance at the present time, when the size of guns is growing so rapidly, was recently read before the Institution of Civil Engineers, by Mr. G. W. Rendel, on "Gun-Carriages, and Mechanical Appliances for Working Heavy Ordnance." Owing to the increase in the power and size of ordnance since the introduction of armour, gun-carriages have gradually become elaborate machines, and the appliances for working the monster ordnance now in contemplation will tax all the resources of mechanical science. The first difficulty experienced in mounting the Armstrong rifled guns arose from the much greater violence of their recoil as compared with that of the old cast-iron guns, a disadvantage mainly resulting from their superiority in lightness, strength for strength. After describing a self-acting brake for arresting recoil, designed and successfully tried at Elswick in 1864, reference was made to the great superiority of wrought-iron over timber as a material for gun-carriages, and experiments made in 1865 were cited as showing the error of the popular objection entertained against wrought-iron on the score of its producing, when struck by shot, more numerous and destructive splinters than timber. By the adoption of mechanical arrangements for the application of manual power to the working of ordnance, guns up to 25 tons weight can now be worked with more ease, safety, and rapidity than guns of a fifth of that weight were formerly worked. Guns, however, are now being made of nearly double that weight, and hence the adoption of some inanimate power, in the place of mere hand labour, for loading and working heavy ordnance, has become an absolute necessity for guns of the future. The simplicity and compactness of hydraulic machinery, and the perfect control it gives over heavy weights, especially adapts it for the purpose. Hydraulic power sufficient for the heaviest gun may be transmitted through a very small tube for long distances and by intricate ways, so that a steam-pumping engine may be placed in a fort or ship in such a position as to be absolutely secure, and supply power by this means for working many guns. The arrangements for loading and working guns by hydraulic machinery embrace a new system of mounting turret-guns, in which the carriage is dispensed with, and the gun is supported on three points, viz., on a pair of trunnions placed well forward, and on a saddle under the breech itself. The trunnion-arms rest in two sliding-blocks, which run in guides on fixed beams, built on the floor of the turret; and immediately behind each block, in the direct line of recoil, are two hydraulic cylinders for checking recoil and running the gun in or out, whilst the saddle which supports the breech slides along a beam or table beneath it. The front of the beam can be raised or lowered by a hydraulic press to give any desired elevation, but the rear is pivoted at a point corresponding to the horizontal position of the gun; consequently it always returns to the horizontal position as it recoils, whatever elevation it may be fired with, clearing the port in coming back. Thus the advantage of muzzle-pivoting, viz., the reduction of the size of port-holes, is

to a large extent realised, without the necessity of lifting or lowering the gun itself.

Ships.—At the recent meetings at the Institution of Naval Architects, several very important papers were read relating to the form, size, and construction of ships. An examination of these in detail would occupy more space than we can now give to the subject, and we shall therefore do little more than name the several principal features of these papers. The prominent position which the question of the proper loading of mercantile vessels has lately assumed, has induced Mr. Benjamin Martell, Chief Surveyor of Lloyd's Registry of British and Foreign Shipping, to investigate the question as to what constitutes a fair freeboard for sea-going vessels, and he has accordingly prepared a scheme of freeboard for the various types of vessels in existence. A set of tables has now been published which determine, not a hard and fast load-line, which would be impracticable, but a fair line of reference for ordinary trades for the various types of sea-going sailing vessels and steamers of the first class. The principle governing this load-line is that of allowing a certain proportion of the total volume of the vessel above the water, in relation to her size and extreme length, and the mode adopted for cutting off this percentage of spare buoyancy is by multiplying the registered tonnage under deck by 100, and dividing this by the product of the length, breadth, and depth of the vessel. The quotient obtained affords a fair approximation to their relative fineness, from which the proper freeboard may be readily traced on the tables.

Mr. W. John's paper on the "Strength of Iron Ships" produces some facts which are sufficient to cause serious alarm, and ought, as he suggested, to secure a very careful consideration of the subject. He remarked, with regard to the constant tendency to increase the size of vessels, that, although it may not astonish anyone to hear that vessels over 300 feet in length are not as a rule strong enough to bear being pivoted on a rock or causeway amidships, it will probably surprise some to learn that they are liable to a strain of 8 tons per square inch afloat. "This," he states, "with the present state of the iron manufacture, gives a factor of safety of not more than 2½, which would scarcely be considered satisfactory by engineers for a land structure."

This paper was followed by one by Mr. W. Froude, F.R.S., Vice-President, on the "Useful Displacement as Limited by Weight of Structure and of Propulsive Power," in which it was remarked that the existing tendency towards extreme length, as compared with midship section, was only justified where extreme speed was required,—speed very much greater than that which sea-going merchant ships in fact realise. He drew a conclusion from prior arguments that the total sectional areas of the deck, the bottom, and the sides of a ship must be proportioned to the stress; and, as the structural weight may be taken as proportioned to their sectional areas multiplied by the ship's length, it must be regarded as proportional directly to the fourth power of the length, directly to the breadth, and inversely to the depth. From this follows the remarkable result that, alike whether we enlarge a ship by increasing her three dimensions throughout in the same given ratio, so as to enlarge her total displacement in the cube of that ratio, or whether we enlarge her by increasing her length alone, the increased structural weight will be as the fourth power of the enlargement of dimensions. It is obvious that in whatever degree the total dead weight of a ship's hull depends on her requirement of structural strength, this conclusion tells most unfavourably on the useful displacement of a ship enlarged simply by elongation, as compared with one enlarged in all her dimensions alike.

Mr. Nathaniel Barnaby, Vice-President, in a paper "On some Recent Designs for Ships of War for the British Navy, Armoured and Unarmoured," entered into particulars of size, form, and constructive details which could not well be given in a condensed form.

Some very interesting experiments for the determination of the resistance of a full-sized ship, at various speeds, by trials with H.M.S. *Greyhound*, were recently instituted by the Admiralty. The conclusion of these experiments was, that a comparison between the indicated horse-power of the *Greyhound*, when on her steam trials, and resistance of the ship, as determined by the

dynamometer, showed that, making allowance for the slip of the screw, which is a legitimate expenditure of power, only about 45 per cent of the power exerted by the steam is usefully employed in propelling the ship, and that no less than 55 per cent is wasted in friction of engines and screw, and in the detrimental reaction of the propeller on the stream-lines of the water closing in around the stern of the vessel.

Railways.—The numerous railway accidents which have taken place of late have necessarily directed the attention of engineers and others interested in the working of railways to the question as to how accidents may be avoided, or, at least, lessened. This subject has been taken up by Captain Tyler, who recently read a paper before the Society of Arts on the "Working of Railways," and on simplicity as the essential element of safety and efficiency in the working of railways. From this paper we learn that the various classes of collision, and the accidents at facing-points, may together be roughly stated to comprise from two-thirds to three-fourths of the more serious casualties to railway trains. Railway working, which was at first easily conducted, is becoming a science with its separate branches, and deserves, therefore, careful study and investigation. Questions as to the arrangement and working of points and signals, and as to preserving intervals of time and space between trains and their accessories, enter more or less into the causes of the majority of railway accidents.

With regard to signals, by the application of locking and other apparatus it is possible to prevent nearly all those accidents which occur in consequence of any mistake of the signalman. Conflict between signals, and conflict between points and signals, may alike be avoided, and a good combination of locking, bar and bolt, may be made to ensure that the facing-points are completely over before the proper signal is lowered, and may also prevent them from being moved during the passage of a train. As regards signalmen, the selection and regular training of fit men for the performance of such duties as they have to perform; the employment of responsible inspectors for constant supervision, and the preservation of rigid discipline; the command of a sufficient number of relieving men to take Sunday duty and to replace signalmen absent from sickness or otherwise, and the maintenance in high condition of the whole of the apparatus, are matters of obvious importance; but experience has shown that it is by no means unnecessary to refer to them. By attention to these considerations, the dangers of facing-points are for the most part obviated, and engines and trains may be turned in and out, and across one another, with marvellous rapidity and facility, and in a way that would be otherwise impossible.

The next branch of the subject is that which refers to the preservation of intervals between trains; and this is best done by what is known as the block system. There are many descriptions of instruments for working the block system, and various rules and regulations applicable to it on different lines of railway. The main principle involved is, simply, by the division of a line into block sections, and by allowing no engine or train to enter a block section until the previous engine has quitted it, to preserve an absolute interval of space between engines and trains.

To sum up the whole case, it is necessary, in railway working, to deal with men and mechanism. Men are fallible, and mechanism may fail. The complications of railway construction and traffic have increased enormously, and are still increasing. At some points, the lines, the sidings, and the crossings are so numerous, and the traffic is so constant, that the employment of the best means and appliances is unavoidable. In other localities, of severe gradients or obstructed view, or when greater danger is otherwise incurred, similar means and appliances are also indispensable. These points and localities become more and more numerous, and ample experience has now been obtained as to the most efficient modes of working. The result of that experience has plainly demonstrated that mistakes and accidents may best be avoided, and efficiency of working may best be attained—(1) By judicious

selection and careful training of the men employed, and especially, in a safety point of view, of engine-drivers and signalmen; (2) by providing those men with reasonable and necessary apparatus and accommodation for the proper performance of their duties; (3) by maintaining good discipline amongst them, which is only feasible when proper means and accommodation are provided, when proper modes of working are adopted, and when it is possible for them to carry out in practice the rules and regulations furnished for their guidance.

Directly bearing upon this subject, also, were two papers read before the Institution of Civil Engineers, the one, "Safety of Permanent Way," by C. P. Sandberg, and the other on "Railway Fixed Signals," by R. C. Rapier. We can now only briefly refer to these two papers. In the former one, the principal object the author had in view was to draw attention to the weakening of rails caused by punching or notching. In the latter, attention was chiefly given to the interlocking and block system, and a tabular statement exhibited showed that, by introducing this system on fourteen of the principal railways, by an expenditure of ½ per cent on the whole cost of the railways, their carrying power might be so increased that three times as many trains could be run on the block system as without it, and with greater safety.

Rock-Drilling.—With the ever-continuing increase in the progress of mining industries, the question of rock-drilling is gradually attaining an importance second to few others connected with the leading industries of the world. A paper on the subject of "Rock-Drilling Plant in its most Recent Modifications," was recently read before the Institution of Mechanical Engineers by Mr. T. B. Jordan, the principal object of which was to direct attention to the Darlington rock-drill, in which most of the defects of other drills were avoided. In this drill all the numerous small parts are omitted, and a machine has been produced that will make from 100 to 1000 blows per minute under a pressure of from 40 to 50 lbs. per inch. Only two parts are used to produce the percussive action of the machine—one fixed, and the other moving. The cylinder and cover may be considered as the fixed part, and the piston and rod—which are forged solid—as the moving part; and these are all that are required to produce the reciprocating action of the machine. The rotation of the tool is obtained by a rifled bar, which is fitted with a ratchet-wheel recessed into the cover of the cylinder. There is no automatic action for advancing the drill to follow up the work, the reason assigned for its absence being that experience has shown that the advantage gained by an automatic arrangement was by no means an equivalent for the extra trouble and expense they entailed.

A new rock-drill, which has for some time past been worked successfully in the United States, has recently been introduced into this country. It is the invention of Mr. Ingersoll, and the main points which constitute its novelty are the giving a rotary motion to a piston—either steam- or air-moved—and conveying that motion to the drill by means of a spiral rod connected to the piston. This rod has an adjustment, so that, when desired, the piston may work direct in one direction and be made to rotate in another. There is also an arrangement of tappets, by which the piston is forced slightly back just before the drill strikes the rock. A cushion is thus formed, which prevents the piston striking the end of the cylinder, and the shock of impact is received only by the drill-piece, and is not communicated to the other parts of the apparatus. The feed-motion of the drill is also governed by a tappet, which is struck by the piston in its forward stroke, and as the drill progresses into the rock.

GEOLOGY.

Physical Geology.—Mr. J. F. Campbell, in a paper on "Polar Glaciation," read before the Geological Society, described his observations made during thirty-three years, and especially those of last summer, when he travelled from England past the North Cape to Archangel, and thence by land to the

Caucasus, Crimea, Greece, and the South of Europe. In advancing southwards through Russia a range of low drift hills occurs about 60° N. lat., which may perhaps form part of a circular terminal moraine left by a retreating polar ice-cap; large grooved and polished stones of northern origin reach 55° N. lat. at Nijni Novgorod, but further east and south no such stones could be seen. The highest drift beds along the whole course of the Volga seem to have been arranged by water moving southwards. In America northern boulders are lost about 39°, in Germany about 55°, and in Eastern Russia about 56° N. lat., where the trains end, and fine gravel and sand cover the solid rocks. Ice-action, in the form either of glaciers or of icebergs, is necessary to account for the transport of large stones over the plains, and the action of moving water to account for drift carried further south. There are no indications of a continuous solid ice-cap following southward over plains in Europe and America to, or nearly to, the Equator; but a great deal was to be found on shore to prove ancient ocean circulation of equatorial and polar currents, like those which now move in the Atlantic, and much to prove the former existence of very large local ice-systems in places where no glaciers now exist.

Prof. Ramsay, in discussing the physical history of the Rhine Valley, stated his opinion that, during portions of the Miocene epoch, the drainage through the great valley between the Schwarzwald and the Vosges ran from the Devonian hills north of Mainz into the area now occupied by the miocene rocks of Switzerland. Then, after the physical disturbances which closed the Miocene epoch in these regions, the direction of the drainage was reversed, so that, after passing through the hill country between the Lake of Constance and Basle, the river flowed along an elevated plain formed of miocene deposits, the remains of which still exist at the sides of the valley between Basle and Mainz. At the same time the Rhine flowed in a minor valley through the upland country formed of Devonian rocks, which now constitute the Taunus, the Hundsruck, and the highland lying towards Bonn, and by the ordinary erosive action of the great river the gorge was gradually formed and deepened to its present level. In proportion as the gorge deepened, the marly flat miocene strata of the area between Mainz and Basle were also in great part worn away, leaving the existing plain, which presents a deceptive appearance of having once been occupied by a great lake.

Palæontology.—A new genus of corals has been founded by Dr. Nicholson, under the name of *Duncanella*, in compliment to Prof. Duncan, of King's College. The specimens of this coral have been found in the Lower Silurian rocks of Indiana.

Mr. L. C. Miall, Curator of the Museum of the Leeds Philosophical Society, has published an excellent guide to the Fossil Collection in the Museum, which is in itself a capital elementary introduction to Palæontological Science: the principal groups of fossils, both plants and animals, are described, and their modes of preservation are pointed out, whilst numerous references are given to works which would prove useful to the student.

Mr. Seeley has recently described a new genus of Plesiosaurians from the Oxford Clay, which he has named *Muræonsaurus*.

A new family of Palæozoic Brachiopoda has been established by Mr. Davidson and Dr. King, which they call the *Trimerellidæ*. As far as is at present known they are confined to the Cambro-Silurian and Silurian systems.

Sub-Wealden Exploration.—A total depth of 671 feet has now been reached, and the boring is still in the Kimeridge Clay. Mr. Topley, who has examined the specimens below the depth of 376 feet, reports that slight indications of petroleum have been noticed all through the clay. He has been able to record no less than twenty forms of Mollusca (the species cannot always be identified) and some fish-remains. *Ammonites biplex*, *Cardium*, *Modiola pectinata*, and *Lingula ovalis* are the most abundant fossils in the Kimeridge Clay. It is not quite certain at what exact depth in the boring the Kimeridge Clay began, but it was probably about 290 feet from the surface, so that about 380 feet of it have been penetrated. Its total thickness at Netherfield (near

Battle) was estimated at 400 feet, so that no doubt the bottom will soon be reached.

The discovery of beds of gypsum between the depths of 100 and 180 feet, in the Purbeck Beds, and which would probably never have been known but for this scientific enterprise, have been already sought for and found at Archer's Wood, on the estate of the Earl of Ashburnham. As Mr. Willett observes, it is a matter of congratulation that by this discovery there has been developed for the county a new industry, which promises to be highly remunerative to all parties.

Geological Society of London.—This Society has just commenced the removal of its Library and Collections from Somerset House to the new apartments prepared at Burlington House. The superior accommodation, which was more especially wanted to display the large collections of rocks and fossils, will no doubt be appreciated by the Members.

The late Professor Phillips.—The scientific world at large, and geologists particularly, have sustained a great loss in the death of Professor Phillips. Few men have possessed so wide a range of knowledge, and no one has done more to further the advance of Geology, by special work in all its departments, than John Phillips. He was born in 1800, and, being early left an orphan, was adopted by his uncle, William Smith, the Father of English Geology. From his sixteenth year he accompanied Smith in his various geological journeys across England, and was thus early trained to accurate observation in the field by his uncle, whose early teachings directed the whole course of his life. In 1825 he was appointed Keeper to the Museum of the Yorkshire Philosophical Society, a post which he held until 1840, when he was appointed to the Geological Survey under De la Beche, and not only did much detailed work in the field, but described the Palæozoic fossils collected in West Somerset, Devon, and Cornwall. In 1834 Mr. Phillips was elected to the Chair of Geology in King's College, London; in 1844 he was appointed Professor of Geology in the University of Dublin; and in 1853 he was called to the duties of Reader in Geology in the University of Oxford. In 1859 he was elected President of the Geological Society of London, and in 1864 President of the British Association, of which latter body he was one of the founders. The writings of Professor Phillips were very numerous. At the time of his death he was preparing a new edition of his "Geology of Yorkshire." He died on the 24th of April, having met with a severe accident on the previous day, falling headlong down a flight of stone stairs.

PHYSICS.

Microscopy.—Mr. F. H. Wenham has described* a more accurate mode of measuring the angular aperture of microscopical objectives than that hitherto practised. The angle of aperture of an object-glass is taken from the focal point through which the rays must pass for all degrees. Those entering at other angles within the plane of focus will give a false indication, from diffused light forming no image. Mr. Wenham employs a movable slit of thin platinum-foil so mounted as to be secure from damage from accidental contact with the object-glass, or otherwise. This instrument is placed upon the stage of the microscope, with the body horizontal, and set on a wooden turntable about 10 inches in diameter, having its edge divided into degrees. The object-glass to be measured is focussed on the glass surface, and the slit adjusted so that its two edges just appear in the field of view, and by the rotation of the turntable the degrees of aperture are read off. The microscope should have a thin stage, so that the rays may not be cut off at extreme angles. Measurements taken with these precautions come out much lower than those by the ordinary mode, as the image-forming rays alone enter into the objective. Mr. Wenham considers the large front glasses of many objectives not only useless but injurious, and the performance of such glasses is much improved by turning away the front, so as to only leave the actual working portion to receive the

* Monthly Microscopical Journal, vol. xi., p. 198.

pencil from the object. Many fine glasses perform indifferently under certain conditions of illumination from the above cause, but, by placing a cap with a suitable aperture to exclude useless light, the definition is rendered perfect. The celebrated American $\frac{1}{6}$th objective, marked as having an angular aperture of 180°, is a remarkable instance of erroneous measurement obtained by receiving false light. The following are the data:—Diameter of front lens, 0·043 of an inch; working distance, 0·013 of an inch. From these measurements, 118° is the utmost that can be obtained, supposing all the light entering takes part in the formation of the image; this glass, measured in the ordinary way, admitted light up to 180°.

A series of more than eighty slides for the magic-lantern, illustrating geology and palæontology, have been published by Mr. James How, of Foster Lane; they are uniform in size with Dr. Maddox's micro-photographs, and are photographed from the best drawings and engravings of various fossils, sections of strata, and sedimentary formations. New photographs are in preparation; the series will prove of great value in illustrating lectures on geological subjects. It is to be regretted that the colour and condition of most geological specimens is such as to prevent good photographs suitable for lantern magnification being taken directly from them.

We have to record the death of Mr. Henry Deane, F.L.S., F.M.S., F.R.M.S., which took place suddenly at Dover on April 11. Mr. Deane joined the Royal Microscopical Society (then the Microscopical Society of London) at its commencement in 1840, and was associated with Messrs. Bowerbank, Bell, Mantell, Quekett, and other pioneers of microscopical science. In 1845 he discovered the existence of *Xanthidia* and *Polythalamia* in the grey chalk of Folkestone, also the now well-known *Arachnoidiscus Japonica* on the sea-weed used by the Chinese for preparing soup. He was also the first to introduce gelatinous mounting media, Deane's gelatine being still employed by microscopists, and considered a most reliable material. It is, however, with the foundation and progress of the Pharmaceutical Society that Mr. Deane's name will be longest remembered. He became one of the Board of Examiners in 1844, and introduced many improvements of a highly practical nature into the examinations, practical dispensing being one of the most notable. He was elected Member of Council in 1851, Vice-President shortly afterwards, and President in 1853 and 1854. Mr. Deane was Chairman of the Committee appointed to assist the College of Physicians in compiling a new Pharmacopœia, and also President of the first British Pharmaceutical Conference. Very few papers were written by Mr. Deane, although his work, both with the microscope and in chemical and pharmaceutical science, was considerable. Attention may, however, however, be called to "Displacement as a Method of Preparing Tinctures, &c.;"* "Experiments on Senna;"† also some on opium preparations and extract of meat, in conjunction with Mr. H. B. Brady.

The subject of cutting sections for the microscope has been very fully discussed at the Quekett Microscopical Club. A large collection of machines were exhibited and described by Mr. E. T. Newton. The points considered most needful to a perfect section-cutter were a true guide-plate, and a very accurate, finely-divided, and tight-fitting screw; a large tube is to be preferred to one of small bore, and the machine should be firmly clamped to the table or bench. Opinions were divided as to the form of the cutting instrument, some being in favour of a knife with a blade broad at the heel and narrow at the point, and others preferring a straight edge; all, however, agreeing that the blade must be so strong that it will not bend under the necessary pressure, as in that case it would dip down and cut an uneven section, besides being liable to come in contact with the edges of the tube and consequently be blunted. It is hardly necessary to mention that success entirely depends upon the knife being extremely sharp. Some experienced operators consider a sharp knife the chief requisite, dispensing with the aid of a machine, and trusting entirely to steadiness of hand. Dr. Hoggan has contrived a machine differing

* Pharm. Journ., vol. i., p. 61. † *Ibid.*, vol. iv., p. 61.

entirely from those on the tube principle almost universally employed. The substance to be cut is attached to a plate worked by a screw somewhat after the manner of the well-known slide-rest of the lathe; the contrivance, which is adapted for cutting sections of either hard or soft substances, has means of keeping the saw or knife rigidly in position while at work. Among the substances used for imbedding tissues to be cut, carrot and elder-pith seemed to offer especial advantages. The process of freezing soft tissues gives good results, and is convenient, because the tedious process of hardening with alcohol or chromic acid is dispensed with; gum-water should be used for imbedding, as water when frozen blunts the knife at the first cut, owing to its becoming crystallised.

HEAT.—M. Berthelot, writing on refrigerating mixtures, says the thermal effect produced when ice is mixed with bihydrated crystallised sulphuric acid is the sum of three effects, viz., the fusion of the acid and that of the ice, which absorb heat, and the combination of the two liquids, which liberates heat. The numbers obtained in practice are under those calculated from theory, owing to loss through radiation. The author shows from theory that no method of cooling is comparable to vaporisation, and he thinks that much higher temperatures may yet be had by means of it.

In prosecuting experiments to ascertain the expansion of various substances by heat, the following experiment was tried:—The bulb of a thermometer was suddenly plunged into melted lead. The mercury instantly darted down far below zero. The action was so quick that the point could not be ascertained. This was caused by the sudden expansion of the bulb by heat before it reached the mercury by conduction; this then began to rise very rapidly, and before it had arrived at the top of the tube the bulb was withdrawn. The experiment requires adroitness, for, as we all know, the instant that the mercury touches the top, the bulb will burst. This must be greased before immersion in the fused lead, otherwise a film of the metal will adhere and retain sufficient heat to carry the mercury to the top with a consequent fracture. A thermometer treated in this rough manner afterwards showed an index error of six degrees, the mercury having risen to that extent; but after a few days the equilibrium was partly restored, and the error remained permanently at three degrees.

Dr. H. Beins in considering the question how to transform heat into mechanical power more advantageously than it is done in our common steam and other engines, found that when natrium-bicarbonate (or the corresponding salt of kalium) in a dry pulverised state or in a watery solution is heated in a closed space, a part of the carbonic acid is given off and condensed in a not heated portion of that space, so that at a temperature of 300° to 400° C. liquid carbonic acid can be distilled out of those salts with a tension of from 50 to 60 atmospheres. The author has experimentally found that a carboleum-engine is easily constructed. Taps and joints can be made to answer perfectly. A year ago he filled a tube of hammered copper with carbonic acid of 50 atm., and not the least loss is as yet observed. Wrought metals are therefore not permeable for gases of that tension. Perhaps the phenomena of porosity, belonging to the common air-pump experiments, are partly caused by surface-condensation. For the great industry, the carboleum-engine can in almost every case substitute the steam-engine. For the small industry, specially for engines working with intermissions and during brief spaces of time, the property of carboleum of being always ready for work is of much importance; for instance, for printing-presses, fire-engines, street-locomotives, &c. By this same property, and since the mechanical equivalent of electricity is very small, a carboleum-engine is a fit and cheap source of electrical light. This method of compression furnishes easily the required tension for the conveyance of letters in tubes, and the modern break apparatus for railways. Perhaps the property of carboleum of possessing a power of projection a hundred times cheaper than gunpowder can be made use of. The fact that a carboleum-engine with sufficient store of carboleum is independent of our atmosphere, makes it possible to construct a vessel, provided with means to sink to any depth of the sea, to rise and sink at will, to cruise about under water, and to maintain the life of the crew during that operation, to develop light, &c. The

importance of this for scientific discoveries and industrial purposes is evident. For the purposes of war, also, must such a small and comparatively cheap submarine vessel place a peculiar, nay a decisive, weight in the scale in the question of our modern ironclads. Freezing machines working by evaporation of carboleum produce ice at a much less cost than any existing freezing apparatus. Regarding this general usefulness of carbonic acid, it is important to call attention to the fact that an inexhaustible store of carboleum is at our disposition in common chalk, for this mineral contains carbonic acid to the amount of half its weight; it can therefore produce twice its volume of carboleum.

ELECTRICITY.—M. Clamond has been improving his thermo-electric pile. He found in it a considerable increase of resistance, and this was due to two causes—(1) Oxidation of the contracts of the polar plates with the crystallised bar under the influence of heat; and (2) splitting of the bar and separation of its different parts in planes perpendicular to its length. In making his couple he uses an alloy of zinc and antimony, and plates of iron as armatures. The bars are collected in crowns of ten bars each, superposed and separated by washers of amianthus, and coupled for tension. The whole forms a cylinder, the interior of which is luted with amianthus, and heated by means of a pipe of refractory earth pierced with holes. The gas mixed with air burns in the annular space between the tube and the bars. The entire surface of the crowns of the pile is 35 square decimetres. The consumption of gas is controlled by M. Girond's regulator. Thus arranged, the pile will work whole months without requiring attention, giving a current absolutely constant. A model exhibited consumed 170 litres, that is, about 5 centimes of gas in the hour, and deposited 20 grms. of copper, which makes the expense of gas per kilogrm. of copper deposited 2 francs 50 cents. M. Clamond has made models of various size; the quantity of electricity increases proportionally to the size of the pieces.

The lever of an electric automatic whistle for locomotives is wrought by an electro-magnet. On passage of a current in a certain direction the magnet lets go the lever, and the whistle sounds. The apparatus is connected by insulating wires with a metallic brush projecting below the locomotive. At a short distance in front of the sight signal there is a metallic plate between the rails, which, when the signal is turned in the position for stoppage, is connected with a source of electricity. On passage of the brush over the plate the current flows and the whistle sounds. It continues to do so till adjusted again by the engine-driver. The Chemin de Fer du Nord have used the system, which is the invention of MM. Lartigue and Forest, eight months, and with satisfactory results.

The following safety electric cable against fires is proposed by MM. Joly and Barbier:—Two wires, insulated from each other by gutta-percha, are corded together into a cable, and are connected at one end with a battery and electric bell. When fire breaks out in any part of the building through which the cable passes the gutta-percha is freed and the wires come in contact, thus closing the circuit and ringing the bell. The condition of the apparatus is tested by means of a peg commutator at the other end of the cable.

TECHNOLOGY.

A new use of glycerin is to prevent the formation of incrustation in steam-boilers. M. Asselin, of Paris, recommends the addition of glycerin in the proportion of 1 kilo. for every 5000 to 8000 kilos. of fuel consumed, on account of its power of increasing the solubility of sulphate of lime, and causing the undissolved portion to be precipitated in a granular form, so as not to adhere to the metal.

As a system of continuous alarum signals to prevent collisions, on railways or at sea, in foggy weather, M. de Mat proposes to compress air in a cylindrical reservoir, from which a tubulure conveys it to three organ pipes (giving *do*, *mi*, *sol*), which can be sounded separately or together. In fog the *do* is sounded, and whenever an engine-driver hears it in an advancing train he sounds his *mi*, the other driver then sounds his *mi* if he is on the right line, then both sound *sol*.

LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

A Manual of Botany, Anatomical and Physiological, for the Use of Students. By Robert Brown, M.A., Ph.D. *Wm. Blackwood and Sons.*

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Divine Revelation or Pseudo-Science? By R. G. Suckling Browne, B.D. *Longmans, Green, and Co.*

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Theory of Arches. By Prof. W. Allan. *New York: D. Van Nostrand.*

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Physiology. By F. Le Gros Clark, F.R.S. *Society for Promoting Christian Knowledge.*

Mineralogy. By Frank Rutley, F.G.S. *London: Thomas Murby.*

The Correlation of Physical Forces. Sixth Edition. By the Hon. Sir W. R. Grove, M.A., F.R.S. *Longmans, Green, and Co.*

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THE QUARTERLY
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I. AN EXAMINATION OF THE THEORIES THAT HAVE BEEN PROPOSED TO ACCOUNT FOR THE CLIMATE OF THE GLACIAL PERIOD.

By THOMAS BELT, F.G.S.

IN the speculations I ventured to make in a recent work* on some of the phenomena of the Glacial period, I purposely avoided entering on the question of the cause of the great accretion of ice, believing that the time was not ripe for its discussion, and hoping that it might be taken up by some astronomer, as it is to Astronomy rather than Geology that we must look for a solution of the problem. I find, however, that my explanations of the facts of the "great ice age" are constantly met by objections founded on the theories of the cause of that event; and I propose in the present paper to discuss the principal hypotheses that have been advanced to account for the origin of the Glacial period, and to endeavour to show that my speculations on the extent and effects of the ice are in accordance with, and a necessary consequence of, the theory that is most in harmony with the facts with which we have to deal.

1. *Theory of a Change in the Relative Position of the Continents and the Ocean.*—In that great work the "Principles of Geology," in which the foundations of the modern science were laid in 1830 by Lyell, and in successive editions in which the veteran philosopher has ever kept abreast of advancing knowledge, he has brought forward and supported the theory that great oscillations of temperature have been produced by changes in the relative positions of land and water. This theory he has enforced with a wealth of illustration derived from his vast acquaintance with geological and geographical facts, and by the masterly arguments of a clear, comprehensive, and judicial mind. Chiefly through

* The Naturalist in Nicaragua, p. 262.

his powerful advocacy, after nearly half a century has elapsed, it still holds its ground amongst the rival views that have been advanced, and deserves our first consideration.

Lyell takes for his starting-point the undoubted fact that the sea and the land are now in some parts changing places. Along some coast lines the land is either slowly sinking or has sunk in post-glacial times, whilst in others the continents have been raised above their former level. He proceeds to show that the climate of a place is greatly dependent upon its position with respect to great masses of land or water; that an insular climate is less extreme than that of the interior of a great continent; and that currents of water from the tropics, or from the arctic regions, are very effectual in raising or lowering the mean temperature above or below what is due to distance from the equator alone. He then considers what change in the relative position of land and water would produce the warmest and what the coldest climate, and comes to the conclusion that if all the land was distributed around the equator we should have the warmest climate possible due to geographical conditions, and that if all the land was situated at and around the poles we should have the extreme of cold.

There can be little doubt that if the second set of conditions prevailed, or even some approach to them, they would be effectual in rendering more severe the climate of polar regions, and in causing a greater accretion of ice than now prevails. A rise of polar and a submergence of tropical lands would certainly lower the temperature of the arctic regions. A mere rise of land, sufficient to close Behring's Straits and to connect America through Newfoundland with Europe, would, by shutting off all warm currents from the polar seas, tend to a greater accumulation of ice, as the heat of the Gulf Stream and other warm currents—that is now expended in tempering arctic seasons and melting polar ice—would then cause greater evaporation, and consequently greater precipitation, on the frozen lands of the north. But it must not be forgotten that the warm currents flowing northwards are counterbalanced by cold ones flowing southwards; and if, on the one hand, regions enjoying a warmer climate through the influence of the Gulf Stream would then be shut off from its influence and subjected to greater cold, so, on the other, coasts—such as that of eastern North America—now cooled by polar currents, would be laved by warmer waters. Yet it is on the eastern side of North

America that the ice extended farthest towards the equator in the Glacial period.

When Lyell first propounded his theory geologists were very imperfectly acquainted with the facts that were to be explained, and it was thought that if it could be shown that by an alteration in the configuration and distribution of land, and a change in the direction of the currents of the ocean, icebergs might be floated down to the latitude of London, lowering the temperature as they do now in South Georgia in lat. 54° S., so as to allow of a perpetual covering of snow and the existence of glaciers on the higher grounds, a satisfactory solution of the problem would be arrived at. But in the half century that has nearly passed since then our conceptions of the extent of the ice of the Glacial period have slowly but greatly expanded, and we know now—although many English geologists still close their eyes to the evidence—that the problem to be solved is not one of icebergs floating over submerged lands, but a vast piling up of ice and snow around the poles, that accumulated until it flowed outwards over the existing continents. Let us trace this great ice-sheet round the northern hemisphere, as we are now nearly enabled to do by the latest observations on its extent in northern Asia.

Commencing in North America, we learn from Dana and other eminent American geologists that to the north of the St. Lawrence the ice was at least 12,000 feet, or $2\frac{1}{2}$ miles, in thickness; in the northern parts of New England was over 6000 feet in thickness, and, gradually thinning southwards, reached in the lower grounds the parallel of 39° N. in the southern parts of Pennsylvania, Ohio, Indiana, Illinois, and Iowa, whilst along the mountain ranges local glacier systems reached in the tropics at least as far as Nicaragua, where within 13 degrees from the equator I found undoubted traces of glacier action reaching to 2000 feet above the sea-level, where snow now never falls.

Coming eastward we find, in Nova Scotia and Newfoundland, everywhere evidence that they were completely overwhelmed with ice. Iceland, according to Robert Chambers, is scored across from one side to the other, and was buried in ice that may have reached the British Isles, for the Hebrides and the north-eastern extremity of Scotland were overflowed by ice that came from that direction. This ice, overflowing Caithness, joined by great streams from Scandinavia, and further reinforced by glaciers from the mountains of Scotland and the north of England, pushed down the bed of the German Ocean, reached as far as the coasts of

Norfolk, and thrust up great masses of chalk and other angular rocks upon the land. We have a measure of its thickness in Southern Yorkshire, and learn that it was not so deep on the eastern as it was on the western side of England, for the drift does not reach higher than 600 feet above the sea, excepting where the Wye, the Calder, and the Aire cut through the Pennine Chain, and form passes through which the ice streamed from the west, where it was much higher. The Irish Sea was filled with it, flowing southwards, at least 2000 feet thick. It butted against the Welsh mountains, and, dividing, one part pushed up the valleys of the Mersey and the Dee, and through what has been called the Straits of Malvern, certainly as far as the water-shed of the Severn, probably as far as the Bristol Channel; the other and larger stream, shouldering the western slopes of the mountains of Cardiganshire, flowed across Anglesea to the Atlantic. In Ireland the ice was still thicker, and Mr. Campbell considers that in the extreme south of that island he has obtained proofs of it having been at least 2000 feet thick. This thickening of the ice westward proves that the British Isles were not glaciated from Scandinavia.

Passing across to the continent we find Scandinavia hugely glaciated, and that the ice-sheet that flowed from it filled the Gulf of Bothnia and the Baltic. Denmark was assailed by the advancing ice, and everywhere traces are left of its vast extent and force. In the island of Möen the chalk strata are dislocated and folded together, inclosing in their folds patches and seams of boulder clay. The Danish geologists have ascribed these to a faulting and bending of the strata since the Glacial period; but both in Nova Scotia and at Abergairn, in Aberdeenshire, I have seen great masses of strata that have been pushed along horizontally over others by the great force of the advancing ice, and think that the post-tertiary contortions of the strata in Möen must be due to the same agency. After crossing the Baltic the ice crept southwards, and all over northern Germany and Holland blocks of stone strew the surface that have been brought by it from the mountains of Norway and Sweden. It reached its southern limit somewhere about Antwerp, and eastward the range of the northern drift has been traced to an irregular line across the Continent. In European Russia the ice reached to Nijni Novgorod, in lat. 52°; to which parallel I have also traced it in north-western Asia, near Pavlodar, in Siberia, and in a paper read before the Geological Society of London have described the facts that have

led me to the conclusion that the ice from the north blocked up the whole water-shed of Siberia as far as the borders of Kamtchatka.

We thus find everywhere in the northern hemisphere that the ice thickened northwards, that it radiated from the pole; and that its margin nearly girdled the world, and probably would be found to have done so completely if there were land to preserve its traces.

There are many geologists who believe that these northern lands were not all glaciated at the same time,—that, for instance, the Glacial period of North America was not contemporaneous with that of Europe. Those who thus argue adopt, in some form or other, Lyell's theory that the cold of the Glacial period was produced by a change in the distribution of land and water. Thus Mr. Hopkins, in 1852, calculated that if—by some change in the relative position of sea and land—the Gulf Stream could be diverted from its present northerly course, whilst northern and western Europe were submerged to the extent of 500 feet, and subjected to the influence of a cold current passing over the depressed area, the snow-line would descend to 1000 feet above the sea-level in Wales and the west of Ireland, and glaciers would reach the sea. Although this amount of change would be totally insufficient to account for the facts of the Glacial period, it may still be useful to point out that not a single scrap of evidence has been adduced to show that the Gulf Stream ever passed over any portion of Europe or America that is now dry land.

Throughout the whole of the Tertiary period the continents appear to have had much the same area and figure as they at present possess. Dana has also pointed out that, even so far back as the Jurassic period, the Gulf Stream exerted the same kind of influence upon the temperature of the North Atlantic as it does now. He considers that the existence of corals in the English oolites proves that the coral reef boundary extended 22 degrees of latitude beyond its present farthest northern point, and believes that the Gulf Stream must have aided in this result. Other facts indicate its existence and influence in cretaceous and tertiary times,—as, for instance, the representatives of the French Faluns on James's River, in North America, denote a cooler climate in lat 37° N. than prevailed at the same time in lat. 47° N. in Western Europe,—whilst in the glacial epoch the extent of the ice in Western Europe and Eastern North America curiously and suggestively conforms with the curve of the present isothermal lines due to its action. Just as

now, the isotherm of 50° F., passing across the south of England near the latitude of London and the Bristol Channel, sweeps south-westwardly across the Atlantic, and reaches to about Baltimore, in North America, so in the Glacial period the margin of the ice, flowing southwards, attained nearly the same limits; indicating that the warm waters from the tropics then, as now, were deflected against the western coasts of Europe by the rotation of the earth, and gave them a higher temperature than the same latitudes on the eastern coasts of America. The sea teems with life, and it is not possible that this current could have flowed over any part of Europe without leaving many memorials of its course behind it. But even if it had been diverted, and a cold current brought icebergs from the Arctic regions past the British Isles, how could that, or any modification of such a theory, cause continental ice to reach the sea-level in lat. 39° in North America? I cannot imagine any alteration of the present coast-lines that could cause a greater curve in the isothermal lines than at present exists in the North Atlantic; and to assume that during the Glacial period the warm and cold currents shifted their position all round the hemisphere, so as to bring every part, at one time or other, within a greater extreme of cold than now anywhere prevails, is to call for an amount of movement in the earth's crust that no evidence warrants nor analogy suggests.

Whilst Lyell, in his latest works,* adheres to his opinion that former changes of climate have been chiefly governed by geographical conditions, he candidly admits that since he first attempted to solve the problem, our knowledge of the subject has vastly increased, and that it has assumed a somewhat new aspect, so that he now considers it probable that astronomical causes may have combined with geographical changes to produce an exaggeration of cold in both hemispheres. The principal of these astronomical theories I shall now take into consideration, but I shall have in the sequel—when I come to show what bearing the facts of the Early Tertiary period have on the discussion—to make some further remarks upon the insufficiency of geographical changes to account for the great oscillations of temperature of which we have geological proofs.

2. *Theory of an Increase of the Ellipticity of the Earth's Orbit.*—Mr. Croll, in a series of papers published in the "Philosophical Magazine," has advocated, with great ability

* Principles of Geology, 1872, pp. 173 and 284.

and learning, and strengthened by laborious calculations, the theory that the cold of the Glacial period and the warmth of other geological epochs were due to great changes in the ellipticity of the earth's orbit. As has long been known, the earth, in its annual course around the sun, does not describe a circle, but an ellipse, and is much nearer to the great luminary in some parts of its course than in others. Astronomers have also proved that the eccentricity of the orbit varies during vast periods of time, and that at its greatest eccentricity—one of which periods happened about 200,000 years ago—the earth in aphelion was nearly 98,500,000 miles distant, whilst now when in aphelion it is about 90,000,000 miles from the sun.

One result of the eccentricity of the orbit, combined with the obliquity of the ecliptic, or the angle that the axis of the earth makes with the plane of its orbit, is, that at present the sun is north of the equator about $7\frac{1}{2}$ days longer than it is south of it. But as at the time the sun is south of the equator the earth is nearest the source of heat, the southern hemisphere receives just as much heat in its shorter summer solstice as the northern hemisphere does in its longer one. Astronomers have calculated the effect of a much greater eccentricity of the orbit, and have unanimously come to the conclusion that the absolute amount of heat received by the two hemispheres would be the same, however great that eccentricity might be. But as the total amount of heat received from the sun is inversely proportional to the shortest diameter of its orbit, it follows that during the periods of greatest eccentricity the absolute amount of heat received by the earth, and distributed equally to the two hemispheres, would be slightly in excess of that received when the eccentricity was much less.

The general conclusion arrived at by astronomers before Mr. Croll examined the problem—including the eminent names of Humboldt, Arago, and Poisson—was that the climate of our globe could not be affected by any possible change in the ellipticity of its orbit. In this opinion Herschel—who at one time thought that great changes of climate might be so produced—appears afterwards to have coincided. Mr. Croll, however, states that in arriving at this conclusion a most important element of the enquiry had been omitted. Fully admitting that the absolute amount of heat received in the two hemispheres would be the same, however great the ellipticity might be, he yet urges that in that hemisphere in which the nights were longest there would be most heat lost by radiation, and

in that way the mean temperature would be greatly lowered.

Mr. Croll puts his theory briefly in these words:—"The southern hemisphere is further from the sun during its winter than the northern, and therefore cools more rapidly. It is, however, nearer to the sun during its summer than the northern, and on this account cools more slowly. The heat thus saved during summer would exactly compensate for that lost during winter were the two periods of equal length; but as the southern winter is longer than the southern summer by more than $7\frac{1}{2}$ days, there is on the whole a greater amount of heat lost during winter than is saved during summer." "The greater length of the winter half year over the summer half, when the eccentricity is near its maximum, would affect the climate in two different ways:—(1), by allowing the ground to cool by radiation to a greater extent than it would otherwise do were the (summer) seasons of greater length; and (2), by lengthening the ice-accumulating period and shortening the ice-melting period. The influence of the first cause upon the glaciation of the country would probably be felt to a considerable extent; but it is to the second that we must attribute the principal effect."* The above was written in 1865, but I cannot find that Mr. Croll has modified his theory in any later writings; and Mr. James Geikie, his colleague on the Geological Survey of Scotland, has, during the present year, in his work "The Great Ice Age," adopted it, and in discussing it has described it substantially as above. Now if it be true that the hemisphere, that has its winter when the earth is farthest from the sun, will have its mean temperature reduced by an excess of radiation; whilst that of the opposite hemisphere will be correspondingly increased, we have certainly a true cause of former great oscillations of climate. Before, therefore, entering on the consideration of some other causes that would, according to Mr. Croll, be brought into action and intensify the effects, it will be well to examine the fundamental bases of the theory.

1st. Would there be more radiation of heat into space, and consequently increased cold, at times of the greatest ellipticity of the orbit in that hemisphere whose winter happened when the earth was furthest from the sun? Mr. Croll, as we have seen, answers in the affirmative, and in the shape in which he puts it it appears as if it would be so. As at that time the number of hours of night in each

* Reader, 1865, p. 631.

year were much more in one hemisphere than the other, it is quite certain that more heat would be radiated during the nights that were longest. But, and this is the fallacy on which it seems to me Mr. Croll's argument rests, the earth radiates heat in the day time as well as at night, and this has not been taken into consideration. The warmth of the day depends on the excess of heat received over what is radiated, not that there is no radiation at that time; and if we take into account the heat radiated during the day we shall find that no more is lost in one hemisphere than the other from that cause. And if the absolute amount of heat received from the sun be equal whatever the amount of ellipticity, and the absolute amount of loss by radiation also equal when we calculate that radiated during the day as well as that during the night, it is evident that the absolute difference between the heat received and the heat lost, or the mean temperature of the two hemispheres due to these causes alone, must be the same whatever the amount of ellipticity of the orbit may be.

2nd. Would the lengthening of the ice-accumulating period and the shortening of the ice-melting period cause a greater accretion of ice? Here again Mr. Croll and his followers answer unhesitatingly in the affirmative, and they put it in this way:—"At the time of greatest eccentricity during the long winter of aphelion, longer by thirty-six days than the summer of perihelion, such an accumulation of snow and ice would have taken place that even the diminished distance between the earth and the sun in summer time would be powerless to effect its removal."* Here, again, I think the argument is based on a misconception. It is not a fact that our winter begins as soon as the sun has passed the autumnal equinox, though what is called the winter solstice does. The nights are longer than the days, but snow does not immediately begin to fall nor water to freeze, and our winter does not commence on the 22nd of September, but several weeks later. In the shorter but hotter summer of perihelion some excess of heat must be stored up in the earth, the sea, and the atmosphere, not to be entirely given up until long after the winter solstice has been entered on. The advocates of this theory affirm that the mean temperature would be lowered because the heat of the short summer would be taken up in melting the ice that had accumulated in winter, but a pound of water in passing from a liquid to a solid state

* The Great Ice Age. By JAMES GEIKIE. 1874. P. 139.

evolves just as much heat as is required to melt it again, and the heat given off in winter by the freezing water is equal to that absorbed when it melts again, so that the mean temperature is not affected.

Again, it is said that clouds would accumulate around the pole with its winter in aphelion. Why they should do so does not appear very clearly, but clouds would receive the rays of the sun on their upper surface, and in some way or other the heat would be utilised in ameliorating the climate; nor should it be forgotten that clouds prevent radiation during the night as well as intercept the sun's rays during the day.

There is, however, a cause not touched upon by Mr. Croll that does act in preventing the excess of heat of summer counterbalancing its diminution in winter where snow covers the ground. It is not because the heat is used up in melting the snow, but because much of it is not so used up but is reflected back into space from the white surface. If it were not for this, snow would nowhere be perennial, but everywhere the heat of summer would dissolve the snows of winter; and if, without taking into account any lengthening of the winter by reason of the ellipticity of the orbit, the whole of the winter solstice were an ice-accumulating period it would now gather year by year until it overwhelmed the temperate zones, because the six months' snow would reflect so much of the other six months' heat that it would not be melted but would gradually accumulate. It does not do so, because only at the very poles are there six months winter and six months summer, and the ice-accumulating period gradually decreases when we leave the poles, and reaches zero long before we arrive at the tropics. These conditions were probably the same at the time of greatest ellipticity, and at the most only a very small amount of heat could be lost by reflection from snow-covered lands more than now: and as at that time—according to the law that the amount of heat received is in inverse proportion to the length of the shortest diameter of the orbit—there would be a slight increase in the absolute amount of heat received from the sun, it is probable that one would counterbalance the other; and I cannot but come to the conclusion that Arago was right when he affirmed that even if the ellipticity of the orbit was much greater than astronomers have shown to be possible, "still this would not alter in any appreciable manner the mean thermometrical state of the globe."

Mr. Croll has sought to strengthen his theory by

endeavouring to show that other physical causes would be brought into operation during a great ellipticity of the orbit which would tend to decrease the temperature of the hemisphere that had its winter in aphelion, and to increase that of the other. The most powerful of these he considers would be a change in the great currents of the ocean by which at present a large amount of heat is conveyed from the tropics to the poles. He maintains that these currents are produced by the trade-winds, and that when the temperature of one hemisphere was reduced and the other increased in the manner and by the causes already discussed, the trade-winds on one side of the equator would be weakened and on the other strengthened, and in consequence the warm currents flowing towards the poles would in one hemisphere be augmented and in the other decreased, if not stopped altogether. For instance, he considers that the Gulf Stream is produced by the action of the trade-winds, and that in case of a great ellipticity of the orbit when the winter of the northern hemisphere happened in aphelion the air would be chilled, whilst that of the southern hemisphere would be warmed, and thus the aërial currents flowing from the poles towards the equator would be altered. Under these circumstances "the winds from the severe wintry north would sweep with much more vigour towards the equator than the opposite winds from the south pole. And hence Mr. Croll contends that with weaker winds blowing from the south the great antarctic drift-currents would be reduced in volume, while the subsidiary currents to which they give rise, namely, the broad equatorial and the Gulf Stream, would likewise lose in volume and force. And to such an extent would this be the case that, supposing the outline of the continents to remain unchanged, not only would the Brazilian branch of the equatorial current go on at the expense of the Gulf Stream, but the Gulf Stream he thinks would eventually be stopped, and the whole vast body of warm water that now flows north be entirely deflected into the southern ocean."* Well may Mr. Geikie say that the effect of the withdrawal from the north of all these great ocean rivers of heated water would be something enormous.

But is Mr. Croll's theory of the origin of the Gulf Stream correct? Is it possible to believe that the great body of water in the Atlantic Basin would be warmed at one end and cooled at the other without some system of circulation

* The Great Ice Age, p. 142.

being set up? If currents in the air are caused by the unequal heating of different portions of it, why should not currents in the ocean be in like manner set in motion? Mr. Carpenter contends, and has illustrated by experiment, that they are; and if he be correct, instead of the Gulf Stream being lessened by the increase of ice in the north, it would be greatly augmented; and I have already shown that there is evidence of its existence and influence in the glacial period.

Another cause that Mr. Croll thinks might be a means of increasing the vicissitudes of temperature produced by the eccentricity of the orbit, is a change in the obliquity of the ecliptic. Accepting the conclusions of some eminent astronomers that the obliquity of the ecliptic can only vary to a small extent, he yet considers that this small amount would cause a great change of temperature; that when the obliquity was at its maximum, or, according to Laplace, $24° 50' 34''$, there would be an increase of temperature at the poles equal to 14° or 15° if they were not covered with ice, but if they were, then the total quantity of ice melted at the poles would be one-eighteenth more than at the present.* On the contrary, when the obliquity was at its minimum, there would be a decrease of temperature at the poles and an increase of the ice covering them. This struck me when I first read it as a most extraordinary conclusion, and I considered it must have been the result of an inadvertence, as it appeared obvious that the effect would be just the reverse of that stated. But I find that Mr. Geikie, in his recent work, follows Mr. Croll in this as in other matters, and states that "if the obliquity of the ecliptic reached a minimum during our glacial epoch, as indeed it must have done more than once, the effect of the great eccentricity and diminished obliquity combined would be to intensify the glaciation of our hemisphere."†

As, in the former argument, I have had occasion to show that the radiation of heat by the earth during the day had been neglected, so in this calculation the all-important fact has been overlooked, that if the obliquity of the ecliptic be increased, the arctic circle will be enlarged and a greater area of the earth's surface brought within the influence of the long arctic night. A diminished obliquity, on the contrary, would lesson the difference in the temperate zones between the length of the night and day, and in so far moderate the extremes of cold and heat in winter and

* Philosophical Magazine, vol. xxxiii, p. 436.
† Great Ice Age, p. 147.

summer. The fallacy of the argument can, however, be best shown by considering what would be the effect of diminishing the obliquity to zero. When the direction of the axis of rotation of the earth became perpendicular to the plane of its orbit, the difference of the seasons of the year would disappear and perpetual spring would reign in the arctic regions. All over the globe there would be twelve hours night and twelve hours day, and no amount of ellipticity of the orbit could have any effect in lengthening the nights and days. Every step in the diminution of the obliquity of the ecliptic would be an approach towards this state of perpetual equinox, and tend more or less to equalise the seasons. The theory of Mr. Croll is based on an assumed exaggeration, by increased eccentricity of the orbit, of the effects of the present obliquity of the ecliptic, and it is startling to find it urged that a decrease in that obliquity would increase the results.

Having thus shown that the foundations of the theory present many points of weakness, I shall next take into consideration the question of how far it is in harmony with the geological facts sought to be explained by it. One of the points insisted upon by Mr. Croll, and which is stated to be in accordance with the facts known to geologists, is that during the greatest eccentricity of the orbit periods of glaciation would alternate with others of great warmth. Whilst one hemisphere was undergoing the extreme rigour of a glacial period, the other would rejoice in a "perpetual summer." And, owing to the precession of the equinoxes by which there is a complete revolution of the equinoctial point in 21,000 years, in half that time the hemisphere that had its winter in aphelion would slowly change until it had it in perihelion. The ice that had been heaped up at one pole would melt away and be piled up at the other. And as the last greatest period of elllipticity occupied, according to Mr. Croll's laborious calculations, about 160,000 years, there would during that time be several complete revolutions of the precession of the equinoxes, so that each hemisphere would have alternately several glacial periods and several warm periods.

To prevent misconception I shall give Mr. Croll's opinion on this question in his own words. He says:—"It is physically impossible that we can have a cold and arctic condition of climate on the one hemisphere, resulting from a great increase of eccentricity, without at the same time having a warm, equable, if not an almost tropical, condition of climate prevailing on the other hemisphere." "If the

Post Pliocene period afforded no geological evidence of a warmer condition of climate in Europe than now prevails, it would be so far a presumptive evidence against the assumption that the glacial epoch resulted from cosmical causes." "If it should actually turn out that there is no such thing as a warm and equable condition of climate somewhere about the time of an ice period, then the whole theory would have to be given up, because a warm period according to theory is just as necessary a result of an increase of eccentricity as a cold period."*

Now not only would the periods of great cold alternate in each hemisphere with periods of "perpetual summer," according to this theory, but as the ellipticity of the orbit approached its greatest eccentricity, warm or genial climates would alternate with colder ones, the extremes becoming more and more marked as the time of greatest eccentricity was neared. We ought therefore to find before the Glacial period evidence of great changes of climate, alternations of warm and cold periods, in the successive faunas, of which we have the records preserved in the Tertiary rocks. Instead of this, there are proofs of the gradual and continual decrease of temperature in Europe from the earliest Tertiary times. According to Lyell, "as we ascend in the series, the shells of the successive groups of strata—provincially called 'crag' in Norfolk and Suffolk—are seen to consist less and less of southern species, whilst the number of northern forms is always augmenting, until in the uppermost or newer groups, in which almost all the shells are of living species, the fauna is very arctic in character, and that even in the 52nd and 54th degrees of north latitude."† And if we go back to earlier Tertiary times than the Crag period, we find all the faunas—back to the very commencement of the Tertiary formations—evidencing warmer and warmer climatic conditions as we recede from the Glacial period. Nor is this evidence confined to the faunas; it is perhaps even better illustrated if we trace the successive floras from the Eocene upwards to the Glacial period. Commencing with the Lower Eocene we find in the London clay the fruits of numerous palms, belonging to genera only now found in the tropics, accompanied by the custard apple, gourds, and melons. These are followed in time by the Bournemouth beds, with subtropical Proteaceæ, numerous fig-trees, the cinnamon, and many other plants and trees, reminding the botanist of parts of

* Philosophical Magazine, vol. xxxvi., page 380.
† Principles of Geology, p. 199.

India and Australia. In the Lower Miocene beds of Switzerland, the flora of which has been wonderfully preserved on the northern borders of the Lake of Geneva, there are still species of fig, cinnamon, palm-trees, and other subtropical vegetation, but with them appear species of poplar, hornbeam, oak, elm, and other trees now characteristic of temperate climes, which are absent from the European Eocene strata, and which indicate a less tropical climate. These beds are succeeded by the Upper Miocene strata of Œningen, still containing many exotic genera, but with a still larger proportion of species that betoken that the climate—though still more equable and warm than at present—was gradually becoming unsuitable for subtropical plants. Coming still higher in the Tertiary series, we find in the Lower Pliocene of Italy that most of the subtropical genera have disappeared, and when we reach the Newer Pliocene deposits the trees and shrubs are those now characteristic of European forests, and suggest that the climate was similar to that at present enjoyed in Europe. Then in England we find the Newer Pliocene beds, with their trees and plants of recent species, as in the Cromer Forest bed, followed by lignite beds at Bure and Westleton, containing *Salix polaris*, now only known within the arctic circle, and *Hynum turgescens*, an arctic moss. M. Nathorst, a Swedish geologist, who has studied these beds, considers that there is a gradual passage from the mild period of the forest bed, probably only a little colder than at present, to severe arctic conditions. These Bure and Westleton beds are succeeded by the till and boulder clay of the Glacial period.

If instead of the successive floras we follow the successive faunas, the land animals or the marine, we have a precisely similar succession of events, a gradual transition from the tropical forms of the Eocene and the subtropical ones of the Miocene through the more temperate species of the Pliocene, up to the arctic shells and mammals that usher in the Glacial epoch. The evidence is complete that points to the gradual cooling of the climate, and there is none whatever to show that there were any alternations of cold and warm periods. It is exactly the same kind of evidence as we should have if we travelled from the tropics along the coast of the continent of America northwards. The plants and land animals on the one hand, the inhabitants of the deep on the other, would gradually change their character; tropical forms would give place to subtropical, these to temperate, and finally, when the far north was reached, arctic species would predominate.

Mr. Croll has pointed out that though we have no evidence to support his theory in the successive faunas and floras of the Tertiary strata, yet in the Eocene beds of Switzerland and the Miocene of the north of Italy there are conglomerates containing large transported blocks of stone. Fully admitting that these were most likely transported by ice, I need scarcely remind geologists that no marine remains have been found with them, and that they were probably deposited in lakes, for although the Miocene boulder beds of Piedmont are more than 100 feet thick they contain no organic remains, and we know that this is a feature of modern glacial lakes. The beds rest also on Lower Miocene strata, mostly of fresh-water origin. To adduce such isolated facts as proofs of the existence of Glacial periods in Early Tertiary times is as logical as it would be to argue that there is now a Glacial period in the tropics because there are glaciers there. It is as if a traveller on the coast of western tropical America, coming in sight of one of the snow-capped summits of the Andes, should contend—although the sea and the land teemed with tropical forms of life—that he was in the arctic regions. Probably throughout geological history there never was a time when some mountain summits did not rise above the limits of perpetual snow, and we may expect to find in every geological formation some ice-borne boulders, without being forced to conclude that it required a Glacial period to transport them. The only safe guides to follow are the fauna and flora preserved in the strata, and even these fail us when we go far back in geological time, for we know not what to call tropical and what temperate forms; but so far as Tertiary rocks are concerned we may accept their evidence, and they prove that there were no oscillations between extreme heat and extreme cold, but a gradual and continuous decrease of temperature from the Eocene up to the Glacial period.

Coming to the Glacial period itself, what evidence have we of the intercalation of that time of "perpetual summer" that is, according to Mr. Croll, a necessary consequence of his theory? The fact most commonly appealed to, both on the Continent and in England, in support of this supposition, is the presence of seams of lignite in Switzerland—as at Dürnten, in the canton of Zurich—resting on a great thickness of boulder clay, and capped by beds of gravel with large erratic blocks. These seams of lignite generally vary from 2 to 5 feet in thickness, but in some parts swell out to as much as 12 feet. I admit that the evidence is conclusive that after the ice—during the great extension of the Swiss

glaciers—had occupied the ground for a long period, it retreated, and peat mosses accumulated in low swampy spots; but I dispute that there is any evidence of a warm climate. Cones of the Scotch and spruce firs, and leaves of the oak, the ash, and the yew, have been found in these deposits, and, as these are all of existing species, Prof. Heer has inferred that the climate was similar to that now experienced in Switzerland. In reality it may have been colder, for all these trees range to more northern latitudes. The bones of the large Mammalia found in the same deposits tell us nothing of the climate, or, at the most, do not throw any further light on the question than is derived from a study of the vegetable remains. All that is proved is, that towards the latter part of the glacial period the ice retreated, and after a long interval advanced again, and covered some great mosses that had accumulated during its retreat. We have had a similar event, though on a smaller scale, in historical times. M. Venetz has pointed out that before the tenth century the Swiss glaciers extended further than they now do, that then for four centuries they gradually melted back, and then again began slowly to advance, and have been ever since gradually regaining their lost territory. If this be so, they must have passed over surfaces on which vegetation grew during their retreat, and if these surfaces were again uncovered we might find leaves of existing Swiss trees in deposits between two sets of Moraine gravels, one of an earlier and one of a later date than when the trees flourished.

Mr. Croll has himself advanced, as a crucial test of his theory, that as whilst one hemisphere was being glaciated the other was enjoying an almost tropical climate, and that as these conditions alternated several times during the period of greatest eccentricity, we ought to find proofs of the existence of these warm periods intercalated with those of greatest cold. And the evidence we require is, not that firs, oaks, and yews grew in Switzerland, as they do now, on moraines, during a temporary retreat of the ice, but of species that now live much further south, having then advanced far northwards. In fact, we want evidence, such as we have seen is so abundant in the Miocene strata, of a subtropical fauna and flora having flourished in Europe in interglacial times, and nothing less is satisfactory, according to this theory. The periods of greatest heat are as necessary a result of the theory as those of greatest cold, and they ought to occur alternately.

I fully believe that if any one takes the trouble to read

this paper, in future years, they will think many of my arguments unnecessary and superfluous; but my contemporaries know what a large amount of acceptance this theory has met with amongst our leading scientific men, many of whom have adopted it as the true cause of the Glacial period. What is required, therefore, at the present time, is a thoroughly exhaustive examination of it, and to the best of my ability I shall make it. The most complete geological evidence is that of the marine shells. They have been more certainly and abundantly preserved than other organisms, and from the earliest Tertiary epoch up to the present time we have an almost continuous series illustrating the successive faunas, and in the interglacial beds they have been much studied. I shall now take the evidence that these last afford us into consideration, and that nothing may be overlooked I shall take my examples from the "Great Ice Age" of Mr. James Geikie, who is one of Mr. Croll's most ardent supporters. First of all, we may dismiss all the Scotch interglacial beds as negatively hostile to the theory, as they either contain no organisms at all, or—in a few cases—some shells of arctic types; nowhere have more southern forms been found than those existing off the present coasts. Coming to England, we have the marine shells of the west coast interglacial beds,—those found on Moel Twyfaen, at Macclesfield, and generally over South Lancashire. I have, in another place, argued that these shells are of older date than the Glacial period, and that they were pushed up out of the bed of the Irish Sea by the great glacier that filled it;* but I need not go into this argument here, as, whatever the evidence may be worth, it is again hostile, and Mr. Geikie admits that "upon the whole the fossils indicate colder conditions than now obtain in the Irish Sea."† On the eastern coast most of the shells that have been found indicate a colder climate, but at Holderness a few fragments of more southern species have been discovered. Messrs. Wood and Harmer, who have described these deposits, admit that they have been transported from some other area; and Mr. Croll has himself, with great acumen, shown how they might have been pushed up out of the German Ocean by the ice that brought over blocks of stone from Scandinavia and thrust them up on the same coast. However, whether brought by currents of water, as suggested by Messrs. Wood and

* Nature, vol. x., pp. 25 and 62.
† Great Ice Age, p. 362.

Harmer, or by ice, as suggested by Mr. Croll, these broken and fragmentary shells—mixed through other transported material evidently ice-borne—are the *débris* of beds older than those in which they are now found. The Foraminiferæ of the same deposits have been examined by Messrs. Crosskey and Robertson: they, like the shells, are much worn, and present a more arctic character, varied by the presence of one or two Tertiary forms.* Altogether it appears that the deposits have been formed by the mixing together of the shells of two or more periods; and we might just as readily infer an arctic climate from the arctic shells and Foraminiferæ as a more southern one from the few fragments of species characteristic of the coralline crag, and which were probably derived from beds of that age in the neighbourhood.

In Ireland the shells found in the drift also indicate a colder climate than the present, and in Scandinavia the only evidence of the warm periods of Mr. Croll's theory, advanced by Mr. Geikie, points in reality to the opposite conclusion; that is to say, beds in Scania, described by M. Nathorst, containing Arctic plants—amongst others *Salix Polaris*, now confined to the Arctic Circle,—which indicate a climate more severe than that of Northern Norway. These and other beds so far north are valuable, as evidence that the ice did not destroy all remains of the vegetation that had flourished in the so-called "inter-glacial period," and if during that time more southern forms had ever advanced northwards we ought somewhere to find their remains.

In North America there is, again, no evidence of a warmer climate having prevailed in inter-glacial times; the marine shells and the vegetable remains all point either to more Arctic conditions, or to a climate not warmer than the present.

But, even if we could bring ourselves to believe that all the remains of the southern faunas and floras had been destroyed by the ice of the Glacial period, whilst the more Arctic forms had been preserved, we ought surely to find some evidence of the warm climates to the south of the limit to which the ice extended. In Sicily are preserved abundance of memorials of the cold climate of the Glacial period, when Alpine ice filled all the lakes of North Italy, covered the plains of Piedmont and Lombardy, and cooled the waters of the Mediterranean so that it was occupied by more

* Introduction to Crag Mollusca. By S. V. Wood, Jun., and F. W. Harmer. P. 22.

northern species of mollusca, such as *Cyprina islandica* and many others. In Southern Sicily a magnificent series of shells have been preserved in rocks rising 2000 feet above the sea. Amongst the latest of these deposits, the northern forms of mollusca appear, and they are nowhere accompanied, followed, or immediately preceded by these tropical species that we ought to find if Mr. Croll's theory be true; to obtain them we must go back to Early Tertiary times, to the Miocene and Eocene periods. These alternations of climate cannot have taken place; it is not possible that all memorials of Arctic faunas and floras in the Eocene and Miocene periods, and all the remains of tropical species in the Glacial period could have been destroyed, whilst in the former case the southern forms, and in the latter the northern, were abundantly preserved. And yet, strangely enough, we are told by the advocates of this theory that it is in harmony with geological facts.

Coming down to post-glacial times, we have in the marine shells only evidence of a gradual amelioration of the climate. Some of the freshwater beds are, however, supposed to indicate that, immediately after the Glacial period, a warmer climate prevailed than we enjoy at present. They only, however, show that it was a more Continental one, which is in accordance with other facts indicating that the British Isles were then joined to Europe by continuous land. I have published my reasons* for believing that a great river, into which flowed the waters of the Rhine, the Thames, the Seine, and many other streams, ran southwards, through what are now the Straits of Dover and the English Channel, as far as, and possibly further than, the Bay of Biscay, at a time when the level of the sea stood much lower than at present. The ice of the Glacial period had then retired from the southern portion of the bed of the German Ocean, but the flow of the waters northwards was still stopped by it, or, as I now think more probable, by the great moraines left across the ocean bed by it. Mr. Godwin-Austen has, in his various classical papers on the post-tertiary beds of the British Channel, shown the great probability that the Straits of Dover did not exist until after the Glacial period, but that a neck or isthmus of land stretched across, joined England to the Continent, and divided the German Ocean from the English Channel. Now, at the height of the Glacial period, we know that the greater part of the bed of the German Ocean was filled with ice, that stretched from Scandinavia

* Nature, vol. x., p. 25.

to the coasts of Norfolk, if it did not extend further south. At this time the southern part of the German Ocean bed must have been occupied by a great freshwater lake whose arms ran up the valleys of the Thames and other rivers. The commencement of the cutting out of the Straits of Dover was, I believe, caused by the overflow from this great lake finding an outlet across the neck of land, which was gradually worn down, and the beds of gravel mantling all the lower hills of the Thames valley were, I think, beaches formed at the successively lower levels at which the lake stood. The ice to the north was now gradually receding, and leaving great banks of moraine rubbish in the old ocean bed, to be ultimately levelled by the sea when it long afterwards returned, and which now form the Dogger and other great submarine banks. At the highest point at which the freshwater lake stood, and which marks the extreme rigour of the Glacial period, we have no organic remains, but many boulders in the beach deposits apparently transported by coast ice. Lower down, the ice had retired a little to the north; the climate was still severe, but the mammoth, the woolly rhinoceros, the musk-ox, the lemming, and other animals fitted to live in an Arctic climate, left their remains in the old beaches. Still lower, we find more southern mammalia coming upon the scene, accompanied by freshwater shells, three of which are not found so far north. I thought formerly that their presence merely intimated a lowering of the lake in autumn, or that the ice had melted so far back that it partly drained around Scotland; but, on fuller consideration, I cannot believe that the hippopotamus came up, or the *Cyrena fluminalis* permanently lived in, water chilled by the melting of Continental ice; and I have come to the conclusion that the ice must have retired so far back that it drained entirely to the north of Scotland, and that it had left a great moraine stretching across the ocean bed, where the Doggerbank now lies, that closed the flow of the southern waters northwards. The Straits of Dover, and probably another barrier much further to the west, had by this time been so far cut through that the rivers stood but little above their present levels when the hippopotamus came up them, possibly only in summer. Then, too, existed great river conditions similar to those under which *Cyrena fluminalis* now thrives in Cashmere and Africa. As some additional evidence in favour of this theory, I may add that one of the three river-shells, *Unio pictorum*, has been dredged off the mouth of the British Channel in the course of the supposed great river.

On the continent of Europe, and in North and South America, no evidence whatever has been found to indicate a sub-tropical climate having prevailed in post-glacial times in the temperate regions of the globe, and I cannot but consider that the issue that Mr. Croll has based on the existence of warm climates having existed about the same time as, and intercalated between, his cold climates, must be given against him. If so, it is fatal to his theory, for he has not one whit exaggerated the importance of the necessity of these oscillations of temperature. If the theory be true, each hemisphere endured the extremes of heat and cold. Just as much as the Glacial period lowered the temperature of any place below what it is now, so must the warm period that came on in about ten thousand years have raised it, and it is a rigorous deduction from the theory that, either in the southern or the northern hemisphere, or both, there must have intervened a great period of warmth as great as that of the Miocene epoch since the countries were glaciated.

There are some other facts to be accounted for that are not, I think, explained by Mr. Croll's theory, but they will be better understood if I take them into consideration under the next theory to be discussed.

3. *Theory of a Change in the Obliquity of the Ecliptic.*—So long ago as 1688, Dr. Robert Hooke drew attention to the evidences of tropical climates having prevailed in Europe, and speculated on changes in the axis of the earth's rotation, or a shifting of the earth's centre of gravity, or a change in the obliquity of the ecliptic. The last theory was a favourite one amongst the older English geologists, but even in these early days received little favour from astronomers, for Newton pronounced against it and declared that astronomy did not countenance the theory that there had been any change in the direction of the earth's axis. The celebrated Laplace investigated the problem of the effect of the attraction of the sun, the moon, and the planets upon the equatorial protuberance, and came to the conclusion that this could only cause a variation in the obliquity to the amount of 1° 21′. More recently, Leverrier has examined the same question, and has arrived at the result that it might vary to the amount of 4° 52′, but not more. The difference between Laplace and Leverrier is a large one, but most geologists have accepted their verdict as decisive, that former great changes of climate could not have been caused by variations in the obliquity of the ecliptic.

But granted that the great geometricians could not have erred very much in their calculations, we may still, without presumption, enquire whether there are not other elements of disturbance besides those they investigated. They assumed in their examination of the problem that the thickening of matter around the equator was a constant quantity, whereas there are evidences of great upheaval and depression in remote ages that may have altered the conditions of the question. The gradual heaping up of ice around the poles in the glacial period must have in some measure diminished the difference between the polar and the equatorial diameters. Many physicists believe that even now an elevation of land around the poles and a depression of land in the tropics is taking place.

The protuberance around the equator is not a regular one, but the equatorial circumference approaches in general outline to an ellipse, of which the greater diameter is two miles longer than the other. At the time the above-mentioned calculations were made the data did not exist for determining the irregularity. To the non-astronomical mind it appears evident that this great difference in the equatorial diameters is an element of great importance in the calculations, and as it was not considered we cannot admit that the problem has yet been decided. The great preponderance of land in one hemisphere, not arranged around the pole of the earth but in a mass whose centre is situated near the English Channel, must also be a disturbing element of no mean importance.

Our knowledge of the other planets teaches us that there is no limit to the obliquity of their axes. In Jupiter the axis is nearly perpendicular to its orbit, so that there is no change in the length of its day. In Saturn the obliquity is 29°, in Mars 30¼°, and in Venus it reaches the extreme amount of 75°, so that its tropics overlap considerably its arctic circle, and there are no temperate zones. The original cause of the inclination of the axes of the planets has never been demonstrated, and until this be done it may be allowable to suppose that changes may occur through the same cause.

Lieut.-Col. Drayson has approached this question in a different manner.* Leaving out altogether the consideration of the cause, he contends that a variation of the obliquity is taking place. He shows that according to the observations of the last four hundred years the obliquity of the

* The Last Glacial Epoch. Chapman and Hall.

ecliptic has decreased, and argues that the pole of the earth instead of describing a circle around the pole of the ecliptic describes a larger one around a point six degrees from that centre. It is admitted, and is indeed an established fact, that the obliquity is less than it was some centuries ago, but the generality of astronomers are agreed that this is owing to the small variation that the calculations of Laplace and Leverrier showed to be possible, and that it is simply a coincidence that the path described by the pole is that of a larger circle around a point a little distant from the pole of the ecliptic. They contend that the pole of the earth does describe a circle around the pole of the ecliptic as a centre, but that the outline of that circle is a waved one, and that during the time that observations have been made the direction of the pole has been down towards the trough of one of these waves, but that it will again rise as much above as it dips below the mean distance from the centre. It is an objection to this theory as well as to that of Lieut.-Col. Drayson that it is assumed that the pole of the earth describes a circle, whereas amongst the heavenly bodies we have no circular movements. All the orbits are ellipses of varying eccentricity, and from analogy we should be led to expect that the pole of the earth would not describe an exact circle. That it does so is entirely theoretical, founded on calculations based on the assumption that the earth's equatorial circumference is a circle, which it is not. Lieut.-Col. Drayson has informed me that though he has assumed the curve to be that of a circle, the earlier observations cannot be sufficiently relied on, and it may be that of an ellipse or of a spiral.

Until astronomers have re-considered this question with the light of our present knowledge of the figure of the earth, geologists should not be prevented from speculating on the possibility of great changes in the obliquity of the ecliptic having caused former great variations of temperature. According to Lieut.-Col. Drayson, the obliquity of the ecliptic has been as much as $35\frac{1}{2}°$. The effect of this was, he urges, the production of the Glacial period. He states that as the arctic circle would then reach nearly to latitude $54\frac{1}{2}°$, there would be an accumulation of snow during the winter; which during the summer, in consequence of the great altitude of the sun, would be melted nearly to the poles, occasioning enormous floods. Now if this really would be the effect of a greater obliquity of the ecliptic, we might at once dismiss it as a possible cause of the accumulation of ice in the glacial period, for it is evident that the

great mass of ice—some thousands of feet thick—that moved down southwards over the northern parts of America, Europe, and Asia, could not have been the production of a single winter. It is possible that this and some other geological speculations of the author have prevented many from taking a favourable view of his theory, and it is of importance to discuss what would be the real effect of a greater obliquity of the ecliptic.

We are able to approach this question provided with data derived from the effects of the present inclination of the axis of the earth to the plane of its orbit. To it is due the varying length of the day throughout the year in the temperate and arctic zones, and the consequent production of the seasons. If the axis, as in Jupiter, were perpendicular to the plane of the orbit, night and day throughout the world would be equal. Every day there would be twelve hours' light and twelve hours' darkness. Each place would have but one season, and eternal spring would reign around the arctic circle. Under such circumstances the piling up of snow, or even its production at the sea-level, would be impossible, excepting perhaps in the immediate neighbourhood of the poles, where the rays of the sun would have but little heating power from its small altitude.

Our summer and winter are therefore due to the present obliquity of the ecliptic, and so also is it that now around the poles some lands are being glaciated, for excepting for that obliquity snow and ice could not accumulate, excepting on mountain chains. The obliquity of the ecliptic does not affect the mean amount of heat received at any one point from the sun, but it causes the heat and the cold to predominate at different seasons of the year. Near the poles there are six months' night and six months' day, but the absolute amount of heat that arrives from the sun is the same as if there were twelve hours' light and twelve hours' darkness every day. The cause of perpetual ice and snow is not, as I have already shown, the cooling of the air by the melting snow in summer, nor the formation of clouds shutting off the rays of the sun. It is, I believe, in consequence of the reflection into space of many of the rays of light and heat that fall on a snow-covered surface, and any cause that tends to increase the amount of snow or to extend the snow-covered area will tend to chill the climate of such parts by occasioning more of the rays of the sun to be deflected and lost. Therefore a long hot summer and a long cold winter are more likely to favour the accumulation of perpetual snow

than a place under exactly the same conditions, where a thermometer exposed to the rays of the sun would register the same amount of heat received, but where the sun rose and set every twelve hours, so that the heat by day and the cold by night were never so excessive.

Thus, if we suppose the earth's axis to have been originally perpendicular to the plane of its orbit, so that it had twelve hours' night and twelve hours' day all over the world, and that from some cause or other the axis began to incline and the inclination gradually to increase, the seasons of the year in the temperate and arctic zones would tend to become more and more distinct. An ever-widening circle around the poles would be covered by snow during the cold winter, and lower the temperature of the summer by reflecting the rays of the sun as long as it lasted ; and if the obliquity increased to a greater amount than at present, so would a greater area be brought under arctic conditions, and an approach be made to the cold of the Glacial period.

The accumulation of snow is dependent on another factor, namely, increased precipitation ; and I doubt if any theory would satisfy the conditions of the case that simply increased the cold of the glaciated regions without providing for an increased evaporation outside these regions, and thus to allow greater precipitation upon them. An increase of the obliquity of the ecliptic satisfies this condition, for whilst on one hand the arctic circle would be extended, so on the other would the tropics ; one part of the temperate zones, that next the poles, would have its mean temperature greatly lowered ; whilst the other, that nearest the equator, would have its temperature raised and become an evaporating area. Thus, supposing Lieut.-Col. Drayson to be right in his theory, that at one time the obliquity was as much as about $35\frac{1}{2}^{\circ}$, the arctic circle would then reach to latitude $54\frac{1}{2}^{\circ}$, and the tropics to $35\frac{1}{2}^{\circ}$, reducing the temperate zones from their present width of 43° each to only 19°, one-half of the decrease being added to the arctic circle and one-half to the tropics. As soon, also, as the ice had extended so far as to shut off the warm currents of the ocean that penetrate nearly to the pole, much of the heat now spent in melting the ice of the arctic circle would be expended in evaporation, and precipitation would be proportionally increased.

Those who have followed me in this short argument will, I believe, admit that an increase of the obliquity of the ecliptic does appear to be sufficient to cause an addition to

the snow and ice piled up around the poles; and we may now inquire if the theory throws any light on other problems of the Glacial period, and is in harmony with the facts of geology. In doing this I shall contrast it with the other cosmical theory. The theory of the greater eccentricity of the orbit requires that the glacial periods of the two hemispheres should be at different times; that of the greater obliquity of the ecliptic, that they should be simultaneous. There is not much evidence available, but what little there is, is in favour of the glaciation of the two hemispheres having occurred at the same time. Thus, there exist glacial conditions at present around the poles, due primarily to the obliquity of the ecliptic, and these conditions are contemporaneous in the two hemispheres. More ice and snow is heaped up within the antarctic circle than at its antipodes, because a greater evaporating area of ocean surrounds it, whilst the arctic regions are almost circled by land that not only lessens the evaporating surface, but intercepts much of the moisture-bearing currents from the south. The snow piled up on the Himalayas, the Alps, and other high northern ranges, is just so much prevented from reaching the arctic regions. That the difference is due to lessened precipitation, and not to a difference of temperature, will be seen if we follow the isotherm of 30° around each hemisphere. We shall find it deviating but little in the southern hemisphere from the line of lat. 60°, being now a little to the north and now a little to the south of it. In the northern hemisphere the isotherm of 30° is much more irregular, sometimes running far to the south, sometimes far to the north, but the mean is again about lat. 60°, proving that if there was as much precipitation there would be as much ice and snow to the north of lat. 60° N. as there is to the south of lat. 60° S. Even now, if all the snow of northern mountain ranges was added to that existing to the north of lat. 60° N., the difference would be greatly lessened, and we should have in both hemispheres a partial Glacial period reaching nearly 30° from the poles, and produced by the present obliquity of the ecliptic. Only on one of the other planets has an accumulation of snow at the poles been proved to exist, namely, on Mars; which, with an obliquity of $30\frac{1}{4}$°, is glaciated at both poles at the same time. So that, judging from analogy, we might expect the glacial period of the two hemispheres to have been contemporaneous.

Many plants and some animals are found, in both the northern and southern temperate zones, separated by the whole width of the tropics, which they cannot now pass;

and Mr. Darwin has explained their presence by supposing that during the glacial period they were driven to the high lands of the tropics by the advancing ice, and that on its retreat they followed it north and south. A glacial period in one hemisphere only would not afford this means of migration; the plants and animals driven south by the northern ice would always have a hot zone to the south of them, which they could not pass.

Another class of evidence that favours the theory of the glacial periods of the two hemispheres having existed at the same time, is that connected with the lowering of the sea-level. Mr. Alfred Tylor, some time ago, advanced the theory that the piling up of ice in the northern hemisphere would lower the level of the ocean 600 feet. Mr. Croll has lately discussed the question,* and comes to the conclusion that, if each hemisphere was glaciated alternately, the level of the ocean would be raised, and not lowered, in the one in which the ice accumulated; by the melting of the ice of the opposite pole and the shifting of the centre of the earth's gravity towards that covered by an ice-cap. Though I cannot agree with Mr. Croll's estimate of the thickness of the ice, and think that it could not possibly have been highest at the pole, I have no doubt that a great lowering of the level of the ocean could not have arisen by the accumulation of ice at one pole, if at the same time that now existing at the other was melted off. But if the glacial periods of the two hemispheres were simultaneous, the water abstracted from the sea and frozen into ice at the two poles, and that impounded in the great lakes of Northern Europe, America, and Asia, by the blockage of the northern drainage of the continents by ice, must have lowered the level of the ocean to a great extent.

In my "Naturalist in Nicaragua" I stated that this decrease in the volume of the ocean could not have been less than 1000 feet. I was thus guarded because we had at that time no proof of the ice having descended from the north upon Northern Asia, and there was no certainty that the Polar basin had been filled with it. Since then I have myself found evidence in Siberia that the Arctic Sea was filled with ice, which was piled up so high that it overflowed the low lands as far as lat. 52° N. Calculating from this data, I find that the lowering of the sea-level—on the supposition that the ice was equal in the two hemispheres at the same time—could not have been less than 2000 feet, and may

* Geological Magazine, July and August, 1874.

have been much more. A glacial period in one hemisphere only would not produce this result, and therefore any evidence that tends to prove that the level of the ocean was greatly lowered in the glacial period is also evidence in favour of the northern and southern ice having been contemporaneous.

Over the whole world the distribution of many insular faunas and floras has been explained by the supposition that the islands were at one time joined to continents near them and to each other, in post-tertiary times. In every case that I have examined, the theory is that the last movement of the land has been one of depression. Thus the land over which the flora of Greenland reached that country from Europe is supposed to have sunk down. The lands connecting England with Ireland and the Continent, during the forest periods before and after the culmination of the glacial epoch; the land connecting Malta with Africa; that joining the Islands of the Malay Archipelago on one side to Asia, on the other to Australia; that connecting the West-Indian Islands with Venezuela and Yucatan; and that uniting Tasmania with Australia,—are all supposed to have been submerged by a sinking of the land, and we have in the same areas no corresponding instances of elevation. Whilst all islands having shallow channels, however broad, separating them from each other and from not distant continents, produce evidence of having been formerly connected in post-tertiary times, on the other hand islands surrounded by deep water are distinguished by peculiar faunas. Thus Madagascar is separated from Africa by a deep sea, and its fauna is wonderfully distinct, though it still shows traces of a geologically remote connection with that continent. The Gallapagos Islands are a still stronger case, for though near together they are separated by channels of great depth, and Darwin found them tenanted by distinct species of reptiles, birds, and plants. If the channels were made dry land, by the lowering of the sea, we easily understand why islands surrounded by deep water did not lose their insular character; but on the supposition that they have been produced by movements of the land, the reason is not obvious why the depressions should have been limited to a certain depth.

All round the British and Irish coasts, and around Western Europe, we have submerged forests passing under the bed of the ocean. Some—as that at Cromer—are older, others newer, than the greatest development of the ice of the glacial period. To allow these forests to have grown, we have to suppose an elevation, and for their submergence

a depression, of the land,—on the theory that it was movements of the earth's crust that brought it above and sank it below the sea. Now, in various places in the south of England, we have marine deposits a little older than the forest bed of Cromer: they occur mostly between the present tide-marks,—never higher than we may suppose the tide to have reached before the Straits of Dover were cut through. Therefore, if the surface of the land has oscillated, it is remarkable that it should have returned to the same level as it stood at before the Glacial period; but such a fact is clearly in unison with the idea that it was the mobile water that had retreated and returned. These submerged forests are not confined to Europe, but are found on the coasts of America,—as in the Bay of Fundy,—betokening that their occurrence belongs to a general and not to a local cause.

Another class of phenomena, usually ascribed to a gradual sinking of the earth's crust, but which might also be produced by the return of the sea to the level it stood at before the Glacial period, is that connected with the growth of coral islands. Darwin's celebrated essay on their formation first proved that they were due to the gradual deepening of the water. Dana, closely following Darwin in his theory, estimates that this deepening of the ocean bed from which the coral islands rise has been at least 3000 feet, and that the subsidence to which he ascribes it extends round one-fourth of the earth's circumference in the Pacific, being indicated by atolls in that ocean for 6000 miles in length and 2000 in width. In the Atlantic he considers that "the Bahamas show by their form and position that they cover a submerged land of large area, stretching over 600 miles from N.W. to S.E. The long line of reefs and the Florida Keys trending away from the land of Southern Florida are evidence that the Florida region participated in the downward movement."*

Nor are these indications of either a subsidence of the land or a rise of the level of the ocean since the Glacial period yet exhausted. C. F. Hartt considers he has found proofs in Brazil that that country stood higher when it was glaciated than it now does.† Dana has argued the same respecting the high latitudes of North America. There is hardly a mountain chain of the world that has not been supposed to have stood higher, to account for the lowering of the snow-level on its sides in the Glacial period. The Himalayas, the Alps, the Caucasus, the Pyrenees, the

* Coral Islands, 1872, p. 366.

† Geology and Physical Geography of Brazil. By C. F. HARTT. P. 573.

mountains of Syria, the Atlas Chain, the mountains of New Zealand, of California and Central America, and many others, show distinct traces of glaciers having descended either on ranges where snow now never accumulates or even falls, or else thousands of feet below the present snow-line. It has by some been considered a simple explanation of these facts, to suppose that each mountain chain was elevated a few thousand feet in the glacial period, and has since sank down. Here the land went up and here it went down, they say, and think they have found a solution, without explaining why it should or how it could have done so. I shall have some more remarks to make on this assumed elasticity of the earth's surface, but now pass on to remark how a general lowering of the sea-level would cause the snow-line to descend on every mountain chain. Mr. H. W. Bates has pointed out to me, what seems perfectly obvious when once noticed, but what had certainly not occurred to me when I first wrote on this subject, namely, that a lowering of the sea-level would produce the same effect upon the climate of any place as a rise of the land to about the same amount as the atmosphere would sink with the sea. I find that Humboldt, in whose writings are found the germs of many later theories, had made the same observation.* I fail to see how glaciers could be produced in the tropics on mountain chains far below the present snow-line in the Glacial period if it was caused by an increase in the eccentricity of the orbit; for that could not affect the mean temperature of the tropics where day and night were equal, and the heaping up of ice at one pole could not lower the sea-level much; but if it was caused by an increase of the obliquity of the ecliptic, the mean temperature of the tropics would be lowered through the path of the sun being lengthened, the snow-line would descend still farther by the lowering of the sea, and still farther from increased precipitation, owing to the greater evaporation that would take place when the shallowing of the sea shut off cold currents from the polar regions. The combination of these factors could not fail to lower the snow-line in the tropics thousands of feet, as we find it to have been lowered in the Glacial period.

The examination of the deltas of the great rivers—the Mississippi, the Ganges, the Nile, and the Po—have shown that there are land-surfaces and freshwater deposits hundreds of feet below the level of the sea. All our English rivers run in old channels now filled up nearly to low-water line, but

* Edinburgh Philosophical Journal, vol. iv., p. 267.

which are excavated in the solid rock for hundreds of feet below it. These all prove that either the land stood higher or the sea lower, and I cannot but agree with Mr. Alfred Tylor, who has ably discussed this question, that the cause is not a local one, but a general lowering of the level of the ocean all over the world in Glacial times.

To these many evidences of a rise of the level of the sea produced by the melting of the ice of the Glacial period, I think I may fairly add the traditions of mankind of one or more great deluges that overwhelmed peopled lands. In America, Africa, and Asia the remembrance of great catastrophes that nearly exterminated mankind in certain regions has been handed down, indistinctly it is true, but with a marvellous resemblance in the traditions preserved in countries of the world far removed from each other. Here, again, I think that such a general explanation as that of the rise of the waters of the ocean submerging low-lying peopled countries—accompanied by earthquake convulsions, such as were likely to be occasioned by the strains on the earth's crust when the ice melted off the mountain tops and the polar regions, and ran down to the ocean beds—is a more likely theory than that the traditions refer to local catastrophes.

We have proofs that man existed even in England before the presumed date of the return of the waters of the ocean. When the great lake that I have mentioned filled the southern part of the bed of the German Ocean, whilst the northern part was still occupied by the retreating ice, man appears on the margin of that lake when it stood about two hundred feet above the present sea-level. He follows its receding shores as the great river running from it cuts through the barriers in the English Channel, and throughout its gravelly beaches his flint implements are found along with the bones of the great mammalia. The lake is gradually lowered until the rivers running into it stand only about twenty feet above their present level, and the hippopotamus and other southern mammalia now come up the great river occasionally; then palæolithic man, and the great mammalia on which he possibly subsisted, disappear together, and the waters of the sea occupies the bed of the German Ocean and the channel of the great river. Not from such rude tribes was, however, the story of the great deluge handed down; but during the Glacial period a belt of higher civilisation seems to have girdled the world on the borders of the northern tropic, and it was probably the remnants of ruined and engulphed kingdoms of that zone from which the traditions have come down.

Mr. A. G. Renshaw has pointed out to me that the melting of the ice of the Glacial period must have occupied thousands of years, and I am quite convinced that it must have done so. The gradual growth of coral islands, and the silting up of deltas filled with fresh-water deposits, cannot be explained if we adopt the hypothesis that the ice was suddenly melted. But we do not require thousands or even hundreds of feet of submergence to overwhelm low-lying tracts of country, and I think we may fairly assume that there would be some sudden rise of the sea-level, scores of feet at least, through the rapid melting of great quantities of ice, as, for instance, when the warm ocean currents from the south first gained access to the Arctic regions, or when the immense fresh-water lakes of northern Europe and Asia, pounded back by the ice, broke through their melting barriers and ran down to the ocean. Marine deposits found alternating with land surfaces in the deltas of the Mississippi and the Po indicate such occasional more rapid advances of the sea. It may be said that I am advancing one theory—that of the lowering of the sea-level during the Glacial period—to strengthen another—that of the production of the Glacial period by an increase of the obliquity of the ecliptic ; but the lowering of the sea is more than theoretical,—it is a necessary consequence of the heaping up of ice around both poles at once, and any evidence that it was greatly lowered in Glacial times is also evidence in favour of the theory of the increase of the obliquity of the ecliptic, which would produce a Glacial period in the two hemispheres at the same time.

Whilst we have thus many indications of a general rise of the sea-level since the culmination of the Glacial period, we have a remarkable exception in a rise of land towards the north and south poles, which is believed to be still in progress. In the southern hemisphere it is certainly still in operation at intervals in the southern extremity of South America and in New Zealand. In the northern hemisphere it has been better observed on account of the greater amount of land around the polar regions. One line of elevation commences in Scandinavia at Stockholm on the eastern, and near Gothenburg on the western, coast, increasing northwards as far as the North Cape, where there are marine post-glacial deposits 600 feet above the sea. The land has been elevated since the Glacial period, for the raised beaches everywhere rest on the boulder clay with transported blocks. Professor Kjerulf, of Christiania, has shown that the highest sea-terraces contain Arctic shells, which indicate that the

movement of elevation had commenced when the waters were much colder than now.* This movement appears to be continued eastward round the Arctic sea, for, according to Wrangel, the land is slowly rising around the northern extremity of Siberia. He notices the occurrence of marine beds containing sea-shells of existing species along with the remains of the mammoth several feet above the sea-level. There is some evidence that the coasts of Scandinavia are still rising.

Another line of elevation runs north from near New York. At Brooklyn the sea-beaches with marine shells occur 100 feet above the sea. This elevation also increases northwards. At Quebec and Montreal it reaches between 400 and 500 feet, and much farther to the north within the arctic circle, on the shores of Barrow's Straits, it has carried up sea-shells of existing species to a height of 1000 feet above the ocean. The movement clearly increases towards the pole. How far it extends westward I do not know, but it decreases eastward from Montreal, and in Nova Scotia I could find no traces of any elevation having taken place.

Against these numerous instances of upheaval we have in northern regions the solitary instance of a depression of part of the coast of Greenland believed to be still in progress; and it is a very suggestive fact that that country is at present enduring intense glaciation and buried in snow and ice piled up mountains high upon it. Seeing, then, that towards both poles, with a single exception, there has been a rise of land, in some parts still going on, in all evidently accomplished since the Glacial period, it is an important enquiry whether the land so raised was above or below the level of the sea in pre-glacial times. Within the arctic circle the evidence is clear that it was not, for nowhere have Tertiary marine shells been found. Nor can it be argued that they may have existed, but have been destroyed by the ice of the Glacial period, for Tertiary land-surfaces and land-plants are abundant and well preserved. This points to the conclusion that like Greenland at present the land around the poles sank down after it was covered by ice, and has been slowly rising since it melted away. It is a legitimate speculation, and one fully warranted by the facts of the case, that the cause of the depression was the piling up of a vast weight of ice around the poles, and the cause of the elevation the removal of that vast weight by the melting of the ice. That the movement of elevation continues in some

* The Terraces of Norway. Translated from the Norsk by Dr. Marshall Hall.

places only shows that the earth is a rigid body and but slowly gives way to great strains. We must, according to Mr. Croll's theory, go back 200,000 years for the height of the Glacial period; but not much more than one-tenth of that time would be sufficient according to the theory of an increase in the obliquity of the ecliptic; and I submit that the shorter interval is more in accordance with the continuance of the polar movements, the facts connected with the progress of civilisation northwards, and the little change there has been in the fauna and flora of the world since the Glacial period.

If our Glacial period was merely the heaping up of ice and snow around the North Pole that now exists on both hemispheres, the result would only be a slight shifting of the centre of gravity of the earth northwards; but if it was contemporaneous in the two hemispheres, as would result from a greater obliquity of the ecliptic, the figure of the earth would be changed, its polar diameter would be lengthened, its mean equatorial diameter shortened, and a series of strains would be set up tending to restore its figure of equilibrium. And if during the Glacial period the shape of the earth had approached that of equilibrium through the sinking down of the land around the poles and the rising of land in the tropics, then, when the ice melted away, the polar diameter would be shortened, the mean equatorial diameter lengthened, and forces would be set in operation, tending to lower the land of the tropics, and raise that around the poles. Therefore I am ready to admit that some part of the deepening of tropical oceans as evidenced by the growth of coral islands and reefs—and especially any now going on—may be due to a sinking of the bed of the ocean; but in doing so I by no means admit that the whole or even the greater part of the 3000 feet or more of depression that has taken place, according to Dana, can be ascribed to that movement. But the whole of the deepening of the sea, both that arising from its surface being raised, and that by portions of its bed being depressed, has, I believe, been caused by the gradual melting of the ice of the Glacial period, liberating the water that had been piled up at the poles, and disturbing the equilibrium of the figure of the earth.

I know that eminent physicists ascribe the movements of the earth's surface to its contraction from secular cooling, and Mr. Mallet has lately ably argued that volcanoes are one of the results of the movement due to that contraction. Without wishing to call in question any theories about the

earth having once been in a state of fusion, I can find nothing to warrant the conclusion that for long geological ages it has cooled in any appreciable degree. Laplace, reasoning from astronomical observations made in the time of Hipparchus, calculated that during the last 2000 years there has been no appreciable contraction of the earth by cooling, for the length of the day has not been sensibly shortened, not even to the amount of 1-300th of a second, so that the contraction of the globe must have been inappreciably small or none at all, as it could not take place without affecting the length of the day. We may therefore ask how an amount of contraction inappreciable in 2000 years can have resulted in the great amount of movement of the earth's crust and the vast volcanic energy now apparent, or why should its tendency be to lengthen the polar and shorten the equatorial diameters ? and are not such movements more in accordance with the cause I have suggested ?

It is true that the earth must radiate heat into space ; but it is not evident that it radiates more annually than it receives from the sun, and if it does not it is not a cooling globe. If earthquakes and volcanoes are the result of movements of the earth's crust—produced, not by contraction, but by the strains set in action by the melting of the ice caps of the Glacial period—so probably is what we call the internal heat of the earth increasing in depth in our mines. The usually accepted theory that the increased temperature in depth is due to a greatly heated or even fluid fused nucleus, slowly giving off its heat towards the surface, does not explain the irregular distribution of the heat. To my mind it is much more conceivable and more probable that the centre of the earth is as cold as space, and that the movements of its upper strata and the heat they give rise to are confined to a comparatively shallow envelope, say not more than 500 miles thick.

The insufficiency of the theory of central heat was strongly impressed upon me when I was studying the facts connected with the frozen soil of Northern Siberia. At Yakutsk the soil—excepting a few feet at the surface which is thawed every summer—is permanently frozen to a depth of about 400 feet. This frozen soil extends to the shores of the Arctic Sea, and in many places the cliffs bordering the rivers are composed of alternate layers of soil and ice. It is in these cliffs that the bodies of the Arctic rhinoceros and mammoth have been found with their flesh still preserved. As Lyell as remarked, since they were entombed, the soil cannot have thawed for a single season or their flesh would have

putrefied. The ice, therefore, is as old as the close of the Glacial period, at which time these great quadrupeds flourished, and at Yakutsk has remained unmelted all that time. It seems impossible that it could have done so to a depth of 400 feet from the surface if the earth was a cooling globe. If, however, the heat of the crust of the earth is due to movements within it, we can understand that in Siberia it may not have been developed to the same extent as in other parts; for, according to the researches of Murchison, that country is situated on an area of great geological stability. According to Von Cotta, it was never below the level of the ocean from the close of the Permian epoch up to the Glacial period; and I have been able to determine that this permanence of level has continued up to the present time, and that the strata of the Steppes are fresh-water deposits, excepting those round the extreme northern extremity of the country.

If, whilst accepting Mallet's ably worked out theory that volcanoes are the result of movements of the crust of the earth, I am right in ascribing these movements—not with him to the secular cooling of the globe—but to the forces tending to restore the equilibrium of the earth's figure, disturbed by the accretion of ice at the poles during the Glacial period and its subsequent liquefaction, it will add another to the many wonderful effects due directly and indirectly to the action of the sun. It was the heat of the sun that raised the water by vapourisation to the level at which it congealed near the poles; and after the earth had approached its normal form by the sinking of polar lands, it was the heat of the sun that disturbed the equilibrium again by melting the snow and ice and allowing it to flow towards the equator. Not only volcanoes but the folding of strata might be produced by these movements of elevation and depression; but I guard myself against expressing an opinion whether or not the earlier and greater geological folds and upheavals might not be due to other causes.

I have now brought forward a great variety of evidence, drawn from very different sources, that points to the probability of the Glacial periods of the two hemispheres having been contemporaneous. Of the two astronomical theories it is in favour of the one founded on a great increase in the obliquity of the ecliptic, for that would cause a heaping up of ice around the two poles at the same time. I shall now turn to the consideration of a most important class of facts only incidentally alluded to in the foregoing pages.

We have not only to account for the cold of the Glacial period, but for its converse—the heat of Early Tertiary times.

The same latitudes that in the era of greatest cold were covered with continental ice, or bore just beyond the reach of the great glaciers the stunted Polar willow and a few Arctic mosses and lichens, where the musk-ox and the Greenland lemming found their northern limit in summer, were at the commencement of the Tertiary period covered with subtropical forests. Palm-trees—of types now restricted to the Moluccas, the Phillippine islands, and Bengal,—with custard-apples, melons, and many another tropical and subtropical plant, flourished in the neighbourhood of Paris and London. Huge animals, resembling but larger than tapirs, roamed in these forests; monkies chattered amongst the trees; great tortoises crawled beneath the rank herbage; sea-snakes, crocodiles, and enormous sharks tenanted the waters. If any of the mammalia had at that time become adapted to live in an Arctic climate, they must have retired to the very Pole to find it.

It is these two extremes of heat and cold with which we have to deal. If we confine ourselves to the attempt of accounting for the cold of the Glacial period alone, we grapple with but half the problem. The climate of the Eocene period was apparently as much warmer as that of the great ice age was colder than the present. The converse of the cause of the one extreme in all probability produced the other. It has been my fortune in other branches of enquiry to find in one hemisphere the solution of a question that had puzzled me in another. For instance, the origin of large masses of gold in the gravel-beds of Australia, in districts where the auriferous lodes contained only fine grains of gold, remained doubtful until I found nearly at the antipodes of the first observation, in Nova Scotia, that the very highest parts of the lodes had in some cases been left undenuded, and that there the gold occurred in large pieces, whilst deeper only fine grains were found. The conclusion was obvious that in Australia the tops of the quartz veins containing the "nuggets" had been worn off and carried down into the valleys. And so, in studying the Glacial question, it was not until I occupied myself with the consideration of its antipodes, the climate of Early Tertiary times, that I laid hold of facts that left in my mind little doubt as to what had been the prime cause of the great oscillations of temperature.

When the subtropical fauna and flora lived in Central Europe as far as 52° N. lat., still nearer the Pole vegetation flourished similar to that which now characterises the milder portions of the temperate zones, and the representatives of

the present flora of Northern Europe lived and throve within about 11° of the Pole. Thus at Spitzbergen, far within the Arctic Circle, in lat. 78° 56′, flourished species of hazel, plane, poplar, lime, and beech trees, and Professor Heer considers that firs and poplars must at that time have reached to the North Pole if there was land there then for them to grow on.

The strata in which this fossil flora is found within the Arctic Circle are believed by geologists to be of Miocene age. This determination is based on the fact that of 137 species of plants found in the Greenland beds, 46, or one-third, are identical with species of the Miocene flora of Central Europe. This fact, however, seems to me rather to be in favour of the different age of the two deposits. It is improbable that so many species should have had such a wide range. We have seen that in the Eocene period Central Europe was occupied by a subtropical fauna and flora. Is it not likely that the time of the greatest heat in Europe was also the time of greatest heat within the Arctic Circle, and that, on the advent of the cooler climate of the Miocene, some of the species that had lived much further north migrated southwards into Central Europe, and took the place of the Eocene flora, for which the climate had then become too cool? In correlating the age of the arctic flora with that of the Miocene of Central Europe, we may be making the same mistake as future geologists would do if they assumed that the beds lying above the Cromer forest lived at the same time as some arctic ones now forming because they contain the same plants, yet the former beds were deposited at the very commencement of the Glacial period. I am not sure that the omission of the consideration of the important part that the varying climates of the Tertiary period played in causing the faunas and floras to migrate from one latitude to another may not have led to an exaggerated opinion of the great length of time occupied in forming the strata. It matters not, however, for my argument whether the arctic flora, of which we have such abundant remains, was of Miocene or Eocene age. What I have to say has nothing to do with the existence of the same species at the same time in Central Europe and in North Greenland, but with the fact that such plants were able to live at all so far north. To avoid any mistake I prefer, however, to speak of them as Early Tertiary.

In a paper on the Miocene flora of North Greenland read before the British Association in 1866, Professor Heer mentioned that more than sixty different species of plants

brought from Atanekerdluk, North Greenland, situated in lat. 70° N., had been examined by him. Amongst the trees, the most abundant is the *Sequoia Langsdorfii*, the nearest living ally of which is the *Sequoia sempervirens*, not now found farther north than lat. 53°, and which requires a mean annual temperature of at least 49° F., and that in winter the thermometer should not fall below 34° F. Cones of a magnolia have been found, proving, as Lyell has remarked, that this splendid tree not only lived, but ripened its fruit within the Arctic Circle. Vines also "twined round the forest trees, and broad-leaved ferns grew beneath their shade."* Some of the trunks of trees observed were thicker than a man's body, and one seen by Captain Inglefield stood upright as it had grown.

Nor, as we have seen, did this Early Tertiary flora end in Greenland, but is found, containing a large number of species of trees, in Spitzbergen, in lat. 78° 56′ N., or about 11° from the Pole. Prof. Heer considers that the winter temperature in Greenland could never have fallen below 34° F., and says—"These conclusions are only links in the grand chain of evidence obtained from the examination of the Miocene flora of the whole of Europe. They prove to us that we could not, by any re-arrangement of the relative positions of land and water, produce for the northern hemisphere a climate which would explain the phenomena in a satisfactory way. We must admit that we are face to face with a problem whose solution in all probability must be attempted, and we doubt not, completed by the astronomer."

Whilst there are many reasons, as I have shown in the first part of this paper, for believing that the mean temperature of England might be greatly reduced by geographical changes, there is nothing whatever to lead us to conclude that the present mean temperature of Spitzbergen could be raised by any alteration of the relative positions of the sea and the land. By the present arrangement a large body of warm water is poured past that island, deflecting the isothermal lines in a great tongue northwards, which embraces it in its apex; and no conceivable geographical re-arrangement could raise its mean temperature more above that due to its latitude than what is effected at present.

No re-distribution of land and water could compensate for the length of the Arctic night. Lyell has speculated on the possibility of the trees living without light for months, and thinks they might survive through the long darkness.

* Student's Elements of Geology, p. 223.

But long nights mean extreme cold: the one cannot occur without the other. The earth rapidly radiates its surface-heat into space, and, if the loss be not compensated for by what is received from the sun, the temperature soon falls far below the freezing-point.

Neither could any possible increase in the eccentricity of the earth's orbit alter materially the length of the Arctic night; nor could the moderate amount of change allowed by astronomers in the obliquity of the ecliptic. Taking their highest limit, the Arctic night in lat. 78° 56′ would still last for three months, during which the sun would not rise above the horizon. It is impossible but that the radiation from the earth during that time would produce intense cold. This long night could be lessened in one way, and one way only,—by a much greater change in the obliquity of the ecliptic than astronomers have yet admitted can have taken place.

In an enquiry of this kind it is well when we can get down to such a crucial fact as is that of the flourishing of many species of large trees so far within the Arctic Circle. It is of far more importance than any or all the arguments I have used about the Glacial period. It admits of but one explanation. The long Arctic nights are caused by the obliquity of the ecliptic, and only by the lessening of that obliquity can they be shortened. There is no reason to believe that this vegetation could have been fitted to endure extreme cold. It belongs to many different genera, and the greater part are not those that are now characteristic of cold regions. For these, according to Heer, we should have to go to the very Pole itself. At the same time, in North Greenland, flourished a flora that could only live where frost was unknown. Many of the same species lived much further south along with subtropical forms that prove that the climate of Central Europe was then both much warmer and more equable than it now is, and Heer considers that the mean temperature of North Greenland would have to be raised at least 29° F. to enable the Early Tertiary flora to flourish there.

I have shown that a great increase in the obliquity of the ecliptic would produce the cold of the Glacial period, let us now consider what would be the effect of a great decrease in that obliquity. Would it tend to produce conditions favourable for the growth of vegetation up to the North Pole? It will simplify the question by investigating how far an entire obliteration of the obliquity would ameliorate the climate of the Arctic regions. The present position of

the axis of Jupiter proves that there is nothing impossible in the supposition that that of the earth may also have been perpendicular to the plane of its orbit. The immediate effect would be the equalisation of night and day all over the world. With twelve hours' sunshine and twelve hours' darkness the seasons would disappear, or rather every parallel of latitude would have but one. At the equator alone would the sun rise directly overhead at noon. In the temperate zones would reign perpetual summer; within the Arctic circle perpetual spring. In Central Europe sub-tropical vegetation might then flourish. In North Greenland the sun every day would rise to a height of 20° above the horizon.

The forms of the continents were very much the same as they are now. It is probable that the Gulf Stream exercised the same sort of influence as it does at present on the climate of the North Atlantic, and it is significant of that influence that the most northern Early Tertiary forests have been found in Spitzbergen, whose shores it now laves. The flora of North Greenland suggests that a branch of the Gulf Stream also flowed up along its western coast. With twelve hours' sunshine, ice could not accumulate in Baffin's Bay, and it is not improbable that some of the warm surface currents of the ocean then found a passaget hrough Davis's Strait towards the Pole. Under such circumstances the west coast of Greenland might have its mean temperature raised as much as Prof. Heer thinks is necessary. During the day twelve hours' sunshine would give it the heat of a mild summer's day, and at night the warm currents flowing past its shores would prevent the occurrence of frost. The sequoia and the magnolia might then flourish and perfect their fruits in North Greenland; and even at Spitzbergen the Gulf Stream might cause frost to be unknown; but there the sun would rise to such a small altitude that the climate would not be warm enough excepting for hardy northern trees.

Whilst the cold of the Glacial period and the heat of the Early Tertiary period might thus be caused by a great increase or a great decrease respectively of the obliquity of the ecliptic, the extreme point to which the ice reached southwards, as in America, and that to which vegetation reached northwards, as in Spitzbergen, were both due to geographical conditions still in existence. In both periods we have evidence that the isothermal lines were deflected far northwards by the Gulf Stream, and that the east coast of America was much colder than the west coast of Europe. The evidence is

overwhelming that the primary causes of these great oscillations of temperature were changes in the direction of the earth's axis; and, fortified by the conditions that we see obtain in the other planets, we may ask astronomers to reconsider the question of the possibility of these changes having taken place. Additional data respecting the exact figure of the earth have accumulated since the problem was last treated. The irregular figure of the earth must affect the result; and it is not probable that the effect of the attraction of the sun and the planets upon an irregular equatorial protuberance can cause a perfectly circular movement of the poles. None of the other movements of the heavenly bodies are circular, and why should this one be? The weak point in Lieut.-Col. Drayson's theory is the assumption that the imaginary line that the pole of the earth traces in the heavens is that of a circle. Through removing the centre of that circle from the point first fixed by other astronomers to another, he accounts for the cold of the Glacial period, but offers no explanation of the heat of the Early Tertiary period. He has, however, informed me that the curve really traced may be that of an ellipse or of a spiral, the time over which accurate observations have been made not being long enough to determine the exact figure. Geology teaches us that the obliquity of the ecliptic has been much greater and much less than it is now, but with the cause of these changes it cannot deal. This must be left to astronomy to decide, and I doubt not that the solution of the question will be attempted, and, notwithstanding its difficulty and intricacy, accomplished.

I have now come to the end of my argument. I have had more than one object in view. Besides trying to make plain what I considered the fundamental cause of the great oscillations of temperature, of which we have such abundant proofs in geology, I desired also to indicate the vast scope of the enquiry that the study of the Glacial period involved. It is not simply a question of scratched blocks and transported boulders. The whole physical geography of the world has been affected by it. Man's early history and his present distribution are intimately connected with it. Not only the valleys and the fiords of the north, but the great plains of Europe and Asia were produced by it. Even the existence of our continents may be due to a succession of Glacial periods that have from the earliest geological times disturbed the equilibrium of the earth's figure, and the volcanoes and the earthquake shocks of the present day may be

occasioned by the slow recovery from the last disturbance of that equilibrium. Viewed in these lights, the history of the Glacial period has yet to be written; and whoever has time and ability to take up the study will find it one of extreme interest.

In treating the subject as I have done I know I must with many have weakened my argument by introducing questions not directly bearing on it. They will turn to some text-book and find it stated that the greater part of England was submerged 2000 feet below the sea in Glacial times; or that the secular cooling of the earth is an incontestable physical necessity; or that in some other way I have propounded a scientific heresy. They will fail to see that the main argument is not affected by these auxiliary theories, and they will decide against me. The human mind falls back on precedent and authority, and an original investigator must expect that every step he takes will be disputed. And in many ways the result is most beneficial, for the theories that survive do so because they have an innate vitality that carries them through all opposition; and those that cannot stand the test soon succumb to the chilling blasts of gusty criticism.

There are others, however, who will consider the argument strengthened and not weakened by these subsidiary speculations, for they know that it is characteristic of a true theory, like that of gravitation or the undulatory theory of light, that it explains numerous facts not originally contemplated when it was first suggested. Many must have been led to adopt the beautiful theory of the origin of species by natural selection, through finding that it afforded welcome help in the solution of problems in natural history besides those that Darwin first sought to explain by it. And I claim for this theory that it shows these signs of truthfulness. It not only explains the grand facts of glaciation and of the Arctic Tertiary flora, but it throws unexpected light on such far removed and seemingly unconnected facts as the traditions of a great deluge, the production of volcanic eruptions, and the growth of coral islands. It is the problem of human knowledge to bring the accumulating facts of the world's history through all time into one consistent and harmonious chain of consequences, and I trust I may in this paper have contributed towards that end.

II. LOSS OF LIFE AT SEA.

By Rear-Admiral FISHBOURNE.

THE loss of life and property on the high seas having grown to such enormous proportions that it became the duty of the Government to enquire whether much of this did not arise from preventable causes, we were not surprised therefore that a Royal Commission should have been appointed, or that they should have presented us with an instructive and highly valuable Report.*

Notwithstanding that, on the whole, the Commissioners have arrived at wise conclusions, their Report bears the marks of having been produced under pressure, the Commissioners working against time, and that some aspects of the subject were new to them. We are glad to give prominence to some of their recommendations, which we think cannot be too strongly insisted on :—

That "the Board of Trade should interfere only when there is ground for suspecting some gross mismanagement, and, whenever the case for detention may appear doubtful, to direct the attention of the ship-owner or manager to the circumstances which have attracted official notice."

"That the Marine Department of the Board of Trade should be revised and strengthened."

Some additional nautical assistance "is requisite for the due performance of the duties now entrusted to the Board. A legal adviser exclusively belonging to the Department is also essential."

"It will be the duty of the Board of Trade to check the negligent and to punish the culpable ship-owner, but it is desirable that these functions should be performed without harassing the great body of ship-owners, who, by their ability and indefatigable energy, have contributed to the prosperity of the Empire." And while they wisely deprecate any transference of responsibility from the ship-owner to the Executive Government, they add—"It is the duty of the ship-owner to keep his ship in a seaworthy condition, and to select competent officers and crew."

While the Commissioners insist upon the propriety of ship-owners being exempt from vexatious and injurious interference, they recommend that their natural and just responsibilities should be enforced, and even made more

* Report of Royal Commission on Unseaworthy Ships.

stringent: thus they say—They "think that in analogy to the principle involved in the eleventh section of the Merchant Shipping Act Amendment, 1871, the ship-owner's liability for damage to property or person should be unlimited in cases where the death of the seaman or the damage to person and property has been occasioned by the ship having been sent to sea in an unseaworthy condition, unless he proves that he—or those to whom he commits the management of his business—used all reasonable means to make and keep the vessel seaworthy. He should also, in these cases, be made liable, under Lord Campbell's Act, to the family of the deceased seamen." They "are also of opinion that any provision in a bill of lading or other agreement, having for its object or effect to avoid or limit the liability of the ship-owner in the cases just referred to, ought to have no legal validity." They "think that the ship-owner should *not* be enabled to recover his insurances, whether under a time or voyage policy, when it could be shown that he or his agent had not done everything reasonably within their power to make and maintain the ship in a seaworthy condition, and that unseaworthiness occasioned the loss."

Fixing such grave responsibility upon the ship-owner makes it imperative on the Government to protect him against that growing spirit of insubordination, carelessness, and ignorance on the part of the crews. With this view the Commissioners recommend enactments and prosecution under them of captains, mates, or men, whose defaults have occasioned the loss of life or property, and they properly recommend—for the owner cannot be well made responsible for acts committed abroad to which he is not privy—that the eleventh section of the Merchant Shipping Act, 1871, should be amended, and be made expressly to extend to the master of the vessel; for it is very important to avoid any doubt that the master who, without justifiable excuse, leaves port with his vessel in an unseaworthy condition, renders himself amenable to the criminal law.

The Commissioners justly attach great importance to instituting, in every case of disaster, a searching and impartial enquiry, to ascertain whether the casualty is owing to the faulty construction of the vessel, to bad stowage, to circumstances connected with the navigation, to the incompetency of officer, or to the neglect or misconduct of the crew.

No mere preventive or repressive measures can wholly meet the real difficulty which largely arises from a low moral standard amongst both seamen and officers, some of

whom have worked their way up from the ranks, and have not had equal advantages with others; therefore it is with unmixed satisfaction that we view the approval given by the Commissioners to the training ships, and the duty of the Government to aid such; and we still further rejoice that the present First Lord of the Admiralty has given official recognition and approval to the view of the Governmental obligation.

In some respects these training ships may not be thought an equally good school for seamanship, but it is a far better nursery for the growth of bone and muscle, and far more for the formation of character, and for establishing moral and religious principles,—the only sufficient guarantee for good service and good citizenship,—while they receive an education which in every particular is germane to the duties they will have to perform when they are fairly embarked in the duties of their profession.

Once more, we rejoice that England is not content to have the residuum of other nations to man her ships, and still less content to play the stepmother with her own offspring, but undertakes to fit them by the tens of thousands to fill fittingly noble duties opening the door to higher destinies.

We do not know whether the Commissioners allude, in their opening remarks, to the agitation originated—perhaps not wisely, yet not without cause—by Mr. Plimsoll, when they say—"Public attention has been eagerly directed to some of the above precautions." Other sources of danger have been altogether unnoticed. "Adding a summary of official enquiries into wrecks and casualties, excluding collisions, shows that from the year 1856 to 1872 inclusive, a period of seventeen years, while 60 ships were known to have been lost from defects in the vessel or in the stowage, 711 ships were lost from neglect or bad navigation."

In this the Commissioners are scarcely fair, for they admit that these statistics are hardly reliable, and on page 4 they prove them to be utterly worthless, for there they prove that during the first nine months of the operation of the Merchant Shipping Act, 1873 (36, 37 Vict., c. 85), out of 286 ships surveyed, 256 were found to be unseaworthy,—234 from defects in the ship or equipments, and only 22 from being overladen."

This return shows how comparatively futile would have been the special provisions intended by Mr. Plimsoll's Act for determining the load-line and limiting the deck loads to summer seasons, while his recommendation for general

inspection would have been intolerable ; and if this army of Inspectors were not more capable than some of those now employed, the results would have been most injurious to the trade and dangerous to the sailor. If Mr. Plimsoll would benefit the sailor and his country he must seek other support than Mr. E. J. Reed's exaggerated statements about vessels being loaded till the water washed over their decks. Mr. Reed joins Mr. Plimsoll in his attempts to prevent deck loads being carried, because of raising the centre of gravity of ships, which is the occasion of their capsizing, and this top weight is further added to in the winter months by the seas that then come over their bulwarks. It requires, however, but a small acquaintance with every-day facts on the Thames to have known that barges and yachts are to be seen with the gunwhales immersed and inclining almost with the water up to their masts, and without a shadow of danger. So, also, the Dutch galliots, on the high seas, allow the water to have free access to many parts of their decks amidships, and this because they have wisely supplied a sufficiency of the cardinal element of safety, viz., stability.

The Commissioners come to the conclusion that "the reserve of buoyancy," which they define to be "the proportion which the capacity of the water-tight and solidly-constructed part of the ship which is above water bears to the capacity of the part immersed," should "mainly guide in the settlement of a load-line." Further on they add—"Any rule of freeboard founded on surplus buoyancy gives to a vessel of light scantling an advantage over a stronger vessel. Thus the inferior ship would by law be allowed to carry the heavier cargo. Such an enactment would not contribute to the safety of life at sea."

"There is no general agreement as to a rate by which the requisite amount of reserve buoyancy could be determined, and it appears that, except under definite circumstances, it is not a determinable problem."

"The proper load-line in each particular case depends not only upon the principal dimensions of the ship, but also upon her form and structural strength, the nature of her cargo, the voyage, and the season of the year." Again, they say—"Discretion as to the proper loading of his ship must be left to the ship-owner, or, under his directions, to the manager, on whom the responsibility rests for sending the ship to sea in a seaworthy condition, which responsibility it is inexpedient to diminish. They have therefore come to the conclusion, not without regret, that we cannot prescribe

any universal rule for the safe loading of all merchant ships."

This is all true so far as it goes, but it is very unsatisfactory that it goes no further. They omit from the list of considerations that should influence the load-line, the ruling one—that of the disposition of the cargo: this wrongly stowed renders nugatory, as regards safety, all care that may have been bestowed upon other points. Then there is a looseness in the use of the term *reserve buoyancy* that ought not to occur; the mere fact that a part of the vessel, as she floats, is above water, by no means proves that this is reserve buoyancy—it may be absolutely without buoyancy, and may be, so to speak, only waiting for an opportunity to take its natural place below. The ship may have become, by bad stowage, what Sir Edward Watkin styled—when speaking of vessels designed by Mr. Reed—a "ship upside down."

Buoyancy, to be efficacious in that degree, must be real. Buoyancy is the hydrostatic pressure that supports any body floating in a fluid. It is equal to the difference between the mean specific gravity of the floating body and the fluid in which it floats multiplied by the displacing volume.

It is clear, from the above exact definition, that unless that portion of the body which is temporarily above water is of less specific gravity than the water in which the body floats, it does not possess buoyancy, and cannot give any support.

We strongly object to the season of the year being taken as a consideration when fixing the line to which a ship should be loaded, as it implies that it is proper that a ship should be less safe in summer than in winter, forgetting that she may cross the Tropics, and thus pass from summer into winter, and that she may, and they do, frequently meet with abnormal seas and winds in summer; besides, ships stowed on such erroneous principles as these cannot be otherwise than proportionably unmanageable and ineffective, and do not bring profit to the owners.

The Commissioners seem to recognise the faultiness of their definition of buoyancy, or rather the erroneous ideas that are current about it amongst those who inculcate that it matters not where the buoyancy is placed, so only it be sufficient in the aggregate; for they say (p. 6):—"Double bottoms for water-ballast are attended with danger, because when the cargo is taken on board these spaces are emptied of water, and this may tend to capsize the ship. There are many cases where the double bottom affords additional

security; any legislation on this matter would therefore be inexpedient."

We fear in too many cases the possible advantage of a double bottom in very remote cases, such as taking the ground, is made the justification of establishing a permanent danger of capsizing, as alluded to by the Commissioners, but further of effecting the danger it provides against, for the existence of an inner bottom leads to scamping the work on the outer, and reducing its strength far below that which it ought to be.

This tendency to capsize was designed to be the normal condition, as we learn, of many of our vessels of war, and evidence has been given concerning some of them, that their outer bottoms were too weak to bear their own weight when grounded, and so they would not have taken a first-class, if any, certificate at Lloyd's.

We are glad to find the Commissioners recognising the danger of light bottoms, and of ill-adjusted deck-loads, and therefore inferentially condemning that system advocated by Messrs. Froude, Reed, and others, of giving ship's little stability, not simply in the abstract, but actually contending that little stability was a prime element in *safety*, than which no system can be less well founded or more dangerous; but we think the Commissioners ought to have gone further, and, instead of saying that they "could not give any universal rule for the safe loading of merchant vessels," should have stated that the cardinal element in a ship's safety and effectiveness is in great stability, and for this their centres of gravity, when loaded, should never be much above the centre of their immersed body. This would dispose of the difficulties of low freeboard and water-line and deck-loads.

The Canadian authorities except deck-loads of deal on the principle, reasonable enough, that they are light as compared with the other part of the cargo. Deck-loads of cotton and of deals even are dangerous when the general cargo is only cotton and light wood, for in such vessels it is nearly impossible with such to get the centre of gravity low enough for safety. Nor is there any danger of uneasiness in recommending so low a centre of gravity in merchant ships, since their proportion of breadth—which is the disturbing and antagonistic element, so to speak, as respects motion—is small, and the tendency is to making it smaller, for the sake of greater cargo-carrying and small masts and small crews.

The Commissioners have properly recommended that responsibility shall be shared out to all concerned in due proportion—captain, mates, crew, and owner,—but, while

insisting that the safety of a ship cannot be secured without care and vigilance be given from her first design to her unloading at the port of destination, they fail to mete out to the architects and builders, who uniformly claim the lion's share of success, any share of responsibility or blame for the failure of designs: and yet most of the great catastrophes lie at their doors, and many of the smaller are due to their misleading, or to their, and that of other peoples', assuming that in dealing with purely nautical subjects they can dispense with nautical experience and knowledge, and can decide all nautical questions without the assistance of seamen. We rejoice that the Commissioners are no parties to the discrediting of nautical knowledge in naval questions.

As Mr. Froude's theories concerning the best mode of preventing the rolling of ships in a seaway have been extensively applied in our navy, it has become a matter of great importance to demonstrate their erroneous character, and to point out the great danger of continuing to apply them.

Let us now examine the principles on which Mr. Froude proceeds.

He alleges that "the effort of stability is the lever by which a wave forces a ship into motion; if a ship were destitute of this stability," he says, "no wave that the ocean produces would serve to put her in motion."

He offers the following explanation and illustration in proof of the correctness of the above view:—

"I regret you did not notice the conclusive experimental proof I exhibited of the fundamental proposition, that the surface of a fluid when dynamically inclined is virtually level to a body which floats on it, and as it really lies at the root of the whole theory of the behaviour of a ship amongst waves, I am anxious that not only my re-statement of it, but reference to the experimental proof of it, should appear also."

Mr. Froude then gives the following as the experimental proof of his fundamental proposition.

"If a shallow dish of water be suspended by three equal strings brought to one point, so that it is level when it hangs at rest, then if the meeting of the strings be taken in the fingers, the dish may be swung about in any direction whatever without spilling the water; indeed, it is impossible to spill the water so long as the suspending lines are kept tight by the operation; and if a stable floating body be placed in the water it will carry its mast at right angles to

the water, whether it owes its stability to deeply-stored ballast, or to a broad plane of flotation."

Now there are other conditions that must be complied with before the floating body can be swung in "any direction whatever;" and it is not impossible to spill the water "so long, merely, as the suspending lines are kept tight by the operator," for the water will inevitably be spilled if the motion be not communicated concurrently, slowly, and equably to the containing vessel and the water: moreover, when the model ship is placed on the mimic sea, it also must be so placed before motion is communicated to the dish and water; for if all are not put in motion at the same time then the motion of the ship and water will not synchronise, for they will be subject to different amounts of force, and "the fundamental proposition" will be seen not to hold good.

The principles contained in the experiment are the same as those which obtain in the milkmaid's trick of swinging her pail of milk over head, and in the practice of the sailor in swinging his sounding lead over his head: in each case sufficient velocity in rotation must be given to overcome the gravitating force, or the milk will be spilled, or the sailor will be advised of his stupidity by a good thump. In the case before us, unhappily, it is the sailor who is punished, because another, the philosopher, ignores the existence of a well-known law.

Mr. Froude, having stated his fundamental proposition that "the surface of a fluid when dynamically inclined is virtually level to a body which floats on it, overlooks the essential difference between the conditions which obtain in his illustration and those which obtain in a ship in a seaway."

Because the floating body on his mimic sea is subject to the like dynamic forces with the water, and moves with it, he assumes that a ship in a seaway "is subjected to the same dynamic force as the wave on which she floats, and might be treated as a surface particle on it," which is simply impossible, as a ship is subject to other forces than those which the sea give out; and though she is subject to the motions of the wave, she does not accept the wave motion, for if she did, her motions would always synchronise with those of the wave, which they never can entirely do.

And notwithstanding that the floating body on his mimic sea would continue to move with the water (if once set in motion with the water, and is not subject to any other force, as a ship amongst waves is), whether the form of body were V- or U-shaped, whether the centre of gravity were

high or low, whether the body had one keel or a dozen, small or large; and this, because there is no motion of the particles of water amongst themselves, nor of the floating body amongst them, other than those produced by the one power; they *all* being moved by the same power at the same time, and to the due extent, according to their relative positions in the orbit of motion, the upper parts or particles moving in the least arcs, though their motions may be modified by the law of gravitation; yet every one of the above changes in form, in weight, and in disposition of the weights, would affect the motions of a ship in a seaway in facilitating or in retarding motion, so that a ship's motions could not synchronise with those of the wave. Moreover, as the motions of the water in his illustration are different from those of water in wave motion, in which the upper parts or particles moving with greater velocity than the lower, there is no analogy to the conditions of the water in the dish; also as the parts of a ship are substantially rigid, and one part occupying space amongst the fast-moving particles, and another part amongst the slow-moving particles, it is impossible that the ship could move wholly with either one set or the other, or with both more than in part, and in some varying degree. Again, while in his illustration the containing vessel, the sea, the floating body, and *all* the water are moving together under the impulse of the centrifugal force; in the case of a ship at sea there is a portion of every ship that is down amongst the unagitated water, *below* wave motion; this portion of the body would resist and retard motion, and prevent a ship from accepting and moving with the waves, if otherwise free or disposed to retain her masts perpendicular, to the slope of the wave. Indeed, were it true we should not have any such cases as ships and boats being rolled over by waves, which are only too numerous, and will be more so in proportion as his ideas on that subject are adopted.

In fact in no case are the circumstances of a ship amongst waves similar to those of the illustrative model, nor can a ship ever "accept the aggregate dynamic conditions of the sea on which she floats," nor still less "be treated" with any regard to truth "as a surface particle of the wave on which she floats."

There are also further limitations on the motions of a ship in a seaway, to which the model in Mr. Froude's illustration is *not* subject.

Thus the amount of a ship's motions are the result of the wind on the sails, or on the hull and masts, or the inertia

of the masts and sides of the hull, together with the ever-varying resistances from onward, and from rotatory motion; while in the case of the model there is but one motor, and one influence ruling, that and gravity being common to both.

Nor can it be conceived with any approach to correctness that the place of the operator's hand, as specified in Mr. Froude's illustration, "holding ever so tightly the strings," can represent the *point* of buoyant suspension of a ship, or even on the principle that her motions are analogous, as they are not, to those of a conical or other pendulum, nor that the three strings represent the lines of buoyant power meeting in the hand of the operator.

These forces, as the ship rolls or is inclined, are continually changing in direction and in amount, as it were, first on one string then on the other; therefore the point of suspension and length of strings are continually changing.

Lastly and conclusively, the conditions of Mr. Froude's illustration require that the ship shall be suspended by three or more strings or something analogous; and not that only, but that the wave or waves on which she partially and temporarily floats shall also be like as in the tray experiment suspended; not that alone, but that each wave as it arrives up to and passes under her shall be likewise so suspended; nor that only, but that the ship, the sea, even to the depth to which any portion of the ship is immersed, and a portion of which is always below the limit of the depth of wave motion, and all altogether shall be supported on three or more strings, and be all moved together and to the same extent by an invisible hand communicating motion from one point *above* the ship; whereas everyone must know, if he knows anything about the motions of ships at sea produced by waves alone, that instead of this imaginary hand or power moving the ship from *above*, the ship is always in a seaway moved from *below* without the intervention of strings, and that the *water* and not strings or a hand or anything analogous is the motor, and which, unlike the hand and strings in the experiment, *never* succeeds in *wholly* imparting its own motion to the vessel floating on it.

There is nothing of that which is required by Mr. Froude's theory, which is therefore a mere fiction; and though there doubtless is a superstructure of superior mathematics built up by him, yet as it is *baseless*, his conclusions could not be otherwise than erroneous and proportionably deceptive, and the adoption of them could only lead to the damage of our ships and danger to the lives, if not also to the death, of some of our seamen; and so it has proved in the sad fate

of the loss of the *Captain*, and in the great danger to which other ships were exposed, as the writings of the advocates of these erroneous views have practically admitted.

Then he says, "The effect of stability is the lever by which a wave forces a ship into motion. If a ship were destitute of this stability no wave that the ocean produces would serve to *put* her in motion, *whether* the stability be due to deeply-stored *ballast* or to the broad plane of flotation."

Yet the effort of stability is originated by, and is *not* the originator of, motion; it is the effort put forth in resisting motion, and increases in amount with each increase of the extent of the motion up to a certain point.

Stability may be obtained either by deeply-stored ballast, or by a broad plane of flotation, or in part by both; but they are unlike, and in some degree opposite in their operation and effects; yet here also Mr. Froude treats them as though they were strictly alike in their operation and in their effects.

No doubt the plane of flotation in proportion as it is wide when the sea is motionless, therefore not a motor, gives a very high degree of stability; but when the water is moved into waves, that same broad plane becomes proportionally *an instrument* but not a motor, by which the waves give much greater motion to the ship, *they* being the power that puts the ship in motion—making her unsteady yet not unstable, as is argued by Mr. Froude, for in that case she would capsize, as his model also would if unstable, and would not preserve its masts perpendicular to the surface of the water, this new hypothesis notwithstanding.

Unquestionably if a ship be without stability, as Mr. Froude suggests, she will be unstable and not steady, but will do the opposite to that stated by that gentleman, viz., "not be rolled by any sea whatever," for she will certainly roll over, as was near being demonstrated in the case of the *Vanguard* and her sister ships; no doubt she will not roll in the sense of rolling from side to side, for being once rolled, and not possessing stability to bring her back to the perpendicular even when the rolling force ceases to act, she will roll over.

Still less is there any tendency in ballast or a low centre of gravity "to *put* a ship in motion;" its action is to preserve the motionless condition of the ship, and it possesses no leverage till by motion it has been pushed out of the perpendicular by some force which is neither that of stability nor of ballast, ballast itself never being even like the broad plane of flotation, an instrument in originating greater

motion; on the contrary, when it is pushed out of the perpendicular, its effort is always to bring the ship back to the motionless condition, and its effort in this direction is greater in proportion to the extent of the disturbance from the perpendicular, a disturbance originated by that other force; and in proportion as the ballast or centre of gravity is lower does it tend to limit the extent of the motion, which is the opposite of the action of a broad plane of flotation.

In proportion as the stability is derived from the weights being placed low, and relatively great as compared with the *smallness* of the plane of flotation, a ship will roll less and less.

Many have seen this fully illustrated in the steadiness or freedom from rolling with which a boathook floats in the midst of waves.

This condition is also well illustrated by a floating target, which remains vertical because of its studiously low-placed centre of gravity and small plane of flotation.

Let anyone take a boathook and load it still further near the hook so as to push it well down into the water, and arrange that the stave or wood handle shall be as small as will consist with floating the added weight if placed in the midst of high waves, it will be found to float, and continue to do so nearly upright, the waves as they pass running up the stave without inclining it further or rolling it, and why?

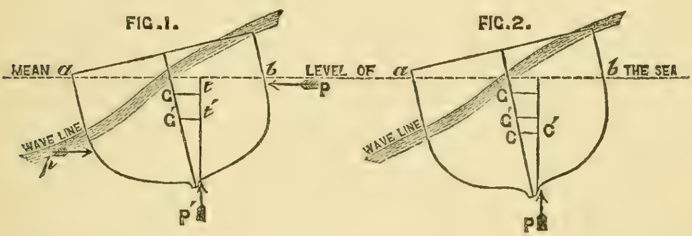
Because the great weight acting low down with all the force of the stability which it gives out, in consequence even of the small incline from the perpendicular, is exercised, not as Mr. Froude suggests, "to put the ship or handle in motion," but to prevent further motion except in the vertical plane; and the ballast succeeds in preventing motion, because the whole plane of flotation, or more properly the volume subject to immersion and emersion as the waves and hollows between the waves pass, and which alone constitutes the instrument through which the waves act to *put* the ship in motion, is little or nothing compared to the force of the low weight, and to the action of the body low down in the undisturbed water, *resisting* motion. The obvious course therefore to be pursued, when it is desired to reduce the amount and the rapidity of motion in ships, would be to keep the plane of flotation comparatively small, the centre of gravity low, and make the depth half the breadth or as great as would consist with general good qualities, and the object for which any particular ship might be destined.

Instead of which two other courses are recommended by Mr. Froude and his school, viz., to raise the centre of gravity

as compared with that in ships generally, and to distribute the weight outwards and on to the sides.

We may examine the effect of such recommendations with advantage.

The condition of ships in a seaway is misunderstood, as is the effect of increasing the inertia of their sides; the condition is not as supposed by Mr. Froude, that of a ship moving with and as a particle in the wave; when waves are large or are steep, and are travelling fast across a ship's path, she has no time and may have very little tendency to accommodate her seat in the water to the surface or level of the wave, so that it becomes oblique to the ship's water-line, as in Fig. 1, producing an inequality in the pressures, which are the cause of the rolling motions. All the conditions of the pressures on the inclined body in still water, shown in pages 73—4 of "Our Ironclads and Merchant Ships," holds good equally as to the upright body with reference to an inclined surface of the wave.



In comparing two ships, or the same under different conditions, we must assume the wave to be alike in all particulars, then consider the variables and their consequences.

The disturbing wave force then is the same, whatever may be the form or distribution of weight in the ship; yet the effect of that force will vary considerably as the above are changed.

We will suppose Fig. 1 to represent the cross section of a ship, which, by weighting her sides, is prevented from inclining with the wave, or from accommodating herself as much as she otherwise would to the surface of the wave, which is represented by the wave line curve.

There is a complete change in the amount of the pressures from those that existed when the sea was level and the ship upright; suppose the new pressures to be represented by the arrows at P P′, and *p*.

The water has accumulated at P, raising the point and increasing the amount of lateral pressure on that side, while

the water has partially left the other side, decreasing the pressure there and lowering the point to p. These two pressures act as a couple to rotate the ship; that at P high up, and more high as the wave is higher and its upper particles moving with greater velocity. That at p low and more low as the hollow of the wave is deeper, and their power to rotate the ship increases in the same extent.

Then P′, moved over to the side of greater pressure, acts up on a line normal to the earth and perpendicular to the mean level of the sea through t; G in the first instance representing the position of the centre of gravity, and G t the perpendicular distance from the line of pressure; this also tends to turn the ship in the same direction as the other pressures.

It is clear that the greater is the accumulation of water the further P′ will be carried over, and the greater will be the lever Gt, which in this case tends to overset; consequently, any arrangements that would tend to increase this accumulation of water would be injurious, and might be dangerous.

Now suppose the centre of gravity lowered to G'_1, then the upsetting lever will be G′ t', which is obviously shorter than G t; consequently, lowering the centre of gravity reduces the power of the wave to incline or roll the ship, and tends to limit the arcs rolled through.

Or suppose, though it is less accurate, these three forces to be represented by one at P passing up through the centre of figure C′. In Fig. 2, it is clear that the greater is the accumulation, the greater distance C′ will be moved out from a line passing through the middle of section or ship; therefore the greater will be the distances of a perpendicular to the earth passing through C′ from the centres of gravity at G or at G′: obviously, as in the former case, in proportion as the centre of gravity G is lowered, so the disturbing lever is decreased.

It is clear also that the greater is the breadth, the greater distance will C′ be carried out, and the greater will be the disturbing lever.

The greater is the accumulation of water also, the further is C′ carried out, and the greater is the disturbing lever. The greater is the effect of Mr. Froude's recommendations, the greater becomes the upsetting lever.

The higher also the centre of gravity is, the longer is the disturbing lever, and the greater is the extent of the motions and danger of upsetting. (See p. 490 for a more definite proof.)

The more rapidly the wave is moving, the greater is the difference between the motions of the upper and lower particles; therefore the higher up is the lateral thrust, and the danger greater from a high centre of gravity.

It may be said that, in lowering the centre of gravity, the point round which the ship turns will be changed, and therefore c′ will not occupy the same place under the altered conditions. This is true, but the fact is in favour of our argument, for, in proportion as the centre of gravity is low, the radius will be shorter, c′ therefore will be moved out a less distance, and the injurious influence of the wave motion will be proportionably less.

To assume, therefore, that the metacentric height is a measure of the disturbing force is erroneous.

It is indispensable for a ship's safety, as it also is for her complete efficiency, that she should possess a considerable metacentric height; then to reduce this by raising the centre of gravity, under an impression that the arcs of roll will be reduced, is erroneous and an unmixed evil.

It is not true, as stated by Mr. Froude, that "the effort of stability is the lever by which a wave forces a ship into motion. If a ship were destitute of this stability, no wave that the ocean produces would serve to put her in motion, whether the stability be due *to deeply stored ballast* or to a broad plane of flotation."

If, however, the metacentric height is made unduly high by great proportionate breadth, or by placing a cargo of metal on the ship's floor, then the motions will be too *rapid,* though short, in which case the evil may be got rid of, with benefit and without danger, by raising the centre of gravity. But still there is a minimum of metacentric height, below which it cannot be reduced with safety.

Ships must yield to the waves, and all arrangements and designs should be to facilitate their doing so slowly and equably. The effect of Mr. Froude's plan is to put off the inclination till the wave accumulates in such force that it will sweep the decks, force the ship over suddenly, and possibly overturn her, if she be not previously "swamped."

Therefore the metacentric height, or the distance of the centre of gravity from the metacentre, must not be taken as giving the measure of the disturbing force, for in lowering the centre of gravity the metacentric height is increased, but we have seen that in proportion as the centre of gravity is lowered so the disturbing action and extent of motion is limited; and therefore the proposition to raise the centre of gravity, with a view to reduce a ship's

motions, is erroneous, and in proportion as it is raised it is dangerous.

No doubt the other proposition, that of increasing the moments of inertia by distributing the weights laterally, if it gives the wave, so to speak, more to do to lift this weight, on the one side, it must be remembered that there is, besides, an accumulation of water on that side which overcomes these moments; but there is a withdrawal of water support from the other side, and that side tends to fall in search of support, and, the more the weights are extended out on that side, the greater is the force from that cause to rotate the ship.

Obviously, their lateral distribution of weight increases and renders a ship's motions in a sea-way dangerous in proportion as the waves are larger and more steep, the cure for which is concentration of weight laterally and distribution of weight downwards.

This has been immemorially the wise practice in bad weather, to send small yards and masts on deck, to fill empty tanks, and get the guns well in from the sides, and notoriously the ships rolled less and were easier, and yet the stability was increased by two causes, viz., lowering the centre of gravity, and increasing the surface stability by reducing the weight of the sides, increasing their buoyant power. Consequently, the only distribution of weight that is safe in bad weather is that which is downwards, and therefore raising weights off the bottom, and concentrating them vertically, is wrong on their own principle, as that reduces the inertia, and doubly wrong on the principle we contend for, viz., the propriety of lowering the centre of gravity and distributing the weights vertically.

The proposition to get rid of the difficulties arising out of the adoption of a wrong principle by increasing the breadth and endeavouring to limit the motions this will entail, by deep keels, will be a failure—

First, it will not effect the object intended. We have passed the limit of practicable depth for useful ships, and any increase of breadth will increase the disparity between depth and half breadth, and occasion, as the ship rolls over and rolls back, a greater rise and fall of the centre of gravity, this action will occasion, so to speak, rolls, independent of those produced directly by the wave by which the total arc of roll will be increased, together with an increase of rapidity of roll, which keels cannot prevent if they will mitigate.

This rise and fall it was that caused the frightful rolling of Sir William Symond's ships.

What will happen in a ship with weight concentrated on her sides, high centre, and low freeboard, say on the L'Aghulas's bank, with a weather current and steep waves, will be that the great inertia of her sides will resist their rising to the wave, but finally it will lift her suddenly, but a large body of water will break on board, and will rush over to the other side. She will then have three forces tending to turn her, the beam sea lifting the upper side, the weight of water weighing down the lower side, and the current running to windward below, making an adverse couple to roll her over, all which will be facilitated by the empty cells in her bottom. If she is not rolled over, it will be that a good Providence has intervened; if she does capsize, we shall know who to hang.

The same danger will arise from a weather tide or current, and from great tidal and solitary waves.

There are two other elements that materially affect the rolling motion, viz., length and depth: the greater these are the greater is the resistance to rotatory motion. Then, in proportion to the height of the waves, the depth of agitated water increases, and so does the portion of the ship down in unagitated water decrease, and with it the limitation to great motion decreases.

It is universally admitted that the lower the centre of gravity is, when once the ship is inclined by a force, the more rapidly will this low centre bring the ship back to the perpendicular, but then, also, in contradiction to Mr. Froude's view, must its power, of necessity, be greater to resist the force of the waves, acting through the solids of immersion and emersion to rotate the ship: also, the wider the plane of flotation, the greater will be the leverage and power of the sea to put a ship into greater motion; and, *cæteris paribus*, the higher the centre of gravity, the less will be the leverage and power of the ballast, or low centre of gravity, to resist motion. Therefore it is that in proportion as the centre of gravity is high, and the vessel broad, the effect of the sea in rolling the ship both rapidly and through large arcs is greater! But obviously it is not desirable that a ship should roll through large arcs, for in that case the armour to be a real protection must cover the whole side thus exposed from time to time at each roll, while, in addition, to a greater degree, would the unarmoured deck be presented to the enemy, and the employment of guns on such a deck would be made proportionably difficult; still less is it desirable that these large arcs should be performed rapidly. Therefore the plane of flotation should not be

greater than is necessary to give the requisite stability without having the centre of gravity too low.

The correctness of the above reasoning was abundantly shown in the behaviour of the ships forming the Channel Squadron in 1871, the large ironclads exhibiting to great disadvantage when compared with the small wooden and unarmoured ships, though it is proverbial that, *cæteris paribus*, the larger the ship, the better the weather she ought to make.

Some of the ironclads rolled as much as 62°, some 56°, others 50°, while the much smaller *Topaze* rolled but 22°.

Again, while the little *Topaze* rolled but 22°, the large and most approved ironclad *Minotaur* rolled 39°, and the large and fine *Northumberland* rolled 38° under similar circumstances.

True, Mr. Reed attributes this difference to the existence of that which he thus admits, though he had previously denied it in former examples, the fact that the *Topaze* had larger masts and yards for her size than the others.

But we may ask if large masts produce such remarkably favourable and, we may add, essential qualities, why not increase the size of the masts of the *Minotaur*, as they would serve other useful purposes in addition? and why have shortened the masts in the *Vanguard* class?

The fact is that the effect of the difference of the size of the masts in mitigating the rolling motions of the *Topaze* was little compared to the disturbing effect of her broad plane of flotation, and but for which she would be easier in fine weather also, as she is so much easier in rough weather than the ironclads. The proportion of breadth to length in the *Topaze* was 1 to 4·7, while in the *Minotaur* it was 1 to 6·7; the proportion of breadth to length of the *Lord Clyde*, which was the vessel that rolled through the largest arcs, was also 1 to 4·7. In fact, the metacentric height of the *Topaze* was greater than that of most of the ships present on that occasion.

The rolling of these ironclads is without parallel amongst wooden vessels, and is only approached by those of the very worst form, and yet the ironclads are, in some respects, of a better form than the wooden ships; the excessive rolling, taking them as a whole, being mainly, though not entirely, due to the concentration of weight on the sides and want of stability, the fact being that no iron-clad possesses near the amount of stability assigned by the ordinary mode of calculating. This arises from the excessive overloading of the solids of immersion and emersion, by the armour and by the

other concentration of weight toward the sides, the consequence of which is that these portions of the ship are deprived of buoyancy, and they afford the ship little or no support; so when she is inclined they tend little or nothing, compared to their volume, to move the centre of buoyancy over to one side or the other, in rolling,—so for want of this support, amongst others, the ship rolls through large arcs, once the sea has overcome the inertia of the sides.

And this evil would increase in each, as they were lightened by the consumption of their coals, provisions, and water, and their stability thus reduced. Great as were the arcs of roll recorded of these ships, the probability is that, with the exception of the *Lord Clyde* or another, very little of their weights were out; so they were in the best condition!—how bad the best!

The loss of the steamship *Tacna* is an instructive comment on the foregoing arguments, and a complete condemnation of the latest of the unsafe theories, *i.e.*, *that* "*a vessel whose stability is greatest when placed bottom upwards may yet be perfectly safe when floating upright.*"

The description of the loss, founded on a "mature consideration," as given by the Court, is "the loss of the ship *Tacna* was due to an excessive loading of her main and hurricane decks, which, with the combination of circumstances adduced in evidence as having arisen at the time of altering course for the port of Los Vilos, caused her to heel over until, falling on her beam-ends, she filled and foundered."

The Court condemned the Captain for "not having made known in a more especial manner to his employers the *crankness* of his vessel, and for not having exercised sufficient care in the amount of deck cargo."

Here is a ship "floating upright" till she comes to turn, when the centrifugal force, which is always greater as the centre of gravity is higher, capsized her. But will any reasonable man say she was safe?

Even a Pacific sea or a squall would have done equally what the centrifugal force did.

We have no question here as to a deficiency in the reserve of stability which a high side is said to give. If we ask, Where was her *reserve* of stability that she so went over? echo answers, Where?

The captain was justly condemned for loading the ship as he did, knowing her to be crank; but he was not responsible for the latter, and he might in some degree be excused when persons who are said to be the greatest authorities on such

subjects say that crankness is a benefit and not a defect, so they have recommended little stability!

The ship was lost, like the *Captain*, because "she was not endowed with sufficient initial stability;" for this the naval architect was primarily to blame, and merited condemnation by the Court as having contributed to the loss of life which occurred. Had the ship been lost within Chilian jurisdiction, it appears the captain also would have lost his life.

The ship never was seaworthy from deficiency in her initial stability, and whether that proceeded from direct design, from a double bottom, and from not making due allowance for that, or from an error in calculation, the naval architect alone was to blame and ought to be held responsible; and it will be useless legislating for the safety of life and property if naval architects are allowed to do as seems good in their own eyes without being held accountable. To say that the deck load capsized this vessel is no exoneration, as the Froude-Reed system provides that ships in a seaway shall have a deck load of the greatest, most generally damaging and dangerous kind, that of water, which will rush to the lower side and guarantee an upset; for in providing so far as they could that a ship shall remain upright in a sea, they provide that the waves shall roll into and over her.

These gentlemen may say, Oh, no, we think there ought to be sufficient stability to enable ships to carry the amount of sail with which they are furnished. Let us consider this:—

The maximum pressure of wind must be assumed, acting at the given leverage occasioned by height and area of sail; in fact, the stability must be equal to the force shown by the wind couple in the case; and more, as a force coming suddenly is said to occasion double the inclination, the stability should be double this; then the stability should be sufficient to bear the ship up against inevitable deck loads of water, or a blow of a heavy sea striking her when she is inclining, such as proved fatal to the *Captain*, because of her small initial stability.

The metacentric height, while that is the accepted mode of estimating stability, ought not to be less than 6 feet for smaller, and 5 feet for larger sailing vessels, somewhat less for steam vessels with little more than fore and aft sail.

It is clear no such quantity was contemplated for the *Invincible* or *Sultan* and a fleet of other men of war, which a deck load of water from a sea or other weight would have cut short in their career while hardly begun if they had not been heavily ballasted; and yet a vessel of war shorn of the

power to carry deck loads would often be useless; thus on an 800-ton steamer it depended to keep up communications with the army in Kaffraria and the colony, to throw in reinforcements, provisions, horses, and munitions of war, carrying sometimes as many as 800 levies with their stores and provisions, and this all on deck. Yet this vessel was proverbially an easy vessel. Numberless instances of the necessity for such a property could be given.

It may be affirmed also with confidence that various exigencies occur when such a power is necessary in merchant ships, such as taking crews off sinking vessels, and yet no such power was provided for in the many ships that have capsized because of their having deck loads.

It may be said that we object *in toto* to double bottoms, though they are necessary, because the thin iron skin is more easily perforated than is the wood planking of wood ships.

We do not object to any proper use of a second or inner bottom, to make iron ships stronger and safer against injuries from taking the ground or from torpedoes, or to facilitate repairs: such ships require it, as their bottoms are far less strong and less calculated to bear injury than those of wood ships; and we doubt not those iron vessels, with deep iron bilge-keels, will be specially liable to damage; nay, more, that these very keels on taking the ground in many cases will serve to tear a large portion of the lower bottom out, if they do not injure the inner or upper bottom through the frames, thanks to the little-stability system smuggled into the navy to "*check* rolling."

We do, however, object to raising these inner bottoms unduly for the purpose of raising the centre of gravity from an idea that it is wise to do so.

We object to making the bottoms of vessels light, on the grounds of cheeseparing parsimony, and then assuming that this makes no difference in the properties of the vessel, that the bottoms are equally heavy, though empty, with all other parts of the immersed body, even when they are crammed full, and then assuming that the results of calculations made on such an hypothesis can be otherwise than erroneous, deceptive, and proportionably dangerous. That they are so we have proved in pages 34 to 44 of "Our Ironclads and Merchant Vessels,"* and need not repeat it here.

We have also demonstrated that in proportion as the spaces

* Our Ironclads and Merchant Vessels, by the Author. Harrison and Sons.

between the bottoms are large and empty it will be necessary for safety to lower the centre of gravity ; it will be necessary, also, with the same object, to lower the centre of gravity in proportion as the sides are more and more loaded and deprived of buoyant power.

The oscillations of wide vessels with heavily weighted sides are liable to carry them beyond the upsetting angle ; for this reason, also, their centres of gravity would require to be kept lower.

It will be asked, What! was there nothing in past experience to justify the adoption of the Froude-Reed system? No facts, no experiments proving its correctness and safety? absolutely none.

The one complaint in which all were for long agreed was, that our ships were deficient in stability, from which they always suffered in comparison with French and Spanish ships. This led to the introduction of Sir William Symond's system, and the success of his ships was entirely due to their greater stability, which they maintained till greater stability was given by his opponents.

His system was exploded because his stability was, for the most part, obtained by a broad plane of flotation ; therefore his ships were more at the mercy of the waves, and their motions were extravagantly extensive ; the more so that they were without a low centre of gravity to limit the range of motion.

When steam was introduced the engines and boilers took the place of ballast in keeping the centre of gravity low, more necessary because of the reduced breadth to obtain speed economically, and necessary because of the great reduction of stability when the latter part of the coals were being consumed.

A lower centre of gravity is necessitated in merchant vessels by the fact that many of them are obliged to carry deck loads.

As great stability and much of it by a low centre of gravity was adopted as indispensable by common consent alike of sailors and naval architects, the result of long extensive and conclusive experiments, and represented by a metacentric height of 5 to 6 feet or more, an architect giving much less to a design would have been justly esteemed as foolish or criminally ignorant. The laws of nature being unchanged, how can it be otherwise now in giving metacentric heights, such as 2·6 feet, 2·3 feet, 2·2 feet, and even 1·5 feet under other much more unfavourable conditions? There was nothing in the facts offered in justification of the course pursued.

As we now propose to show:—Mr. Reed wrote, "the steadiness at sea of our ironclads is due to their want of stiffness or stability;" also that it had been found that raising the centre of gravity tended to "check rolling, and that it was a popular fallacy to suppose the armour-made ships roll."

It was recommended by Mr. Froude with a view to reduce the tendency to roll to raise the centre of gravity of ships, and to distribute their weights towards their sides.

Sir Spencer Robinson accepted the principle involved in these statements and writes:—"It is remarkable, that according to theory, the rolling of the ships being very much influenced by the position of the centre of gravity with regard to the metacentre, and by the moment of stability, the order of rolling of the French ships as observed follows that law."

The heat of the metacentre above the centre of gravity in these ships is as follows:—

Armoured	*Solferino,*	4·5 ft.	Armoured	*Couronne,*	5·37 ft.
Unarmoured	*Napoleon,*	4·9 „	„	*Invincible,*	6·36 „
Armoured	*Magenta,*	5·0 „	„	*Normandie,*	6·59 „
Unarmoured	*Tourville,*	5·31 ft.			

"This," he says, is "precisely the order of merit they have taken as to rolling." The *Solferino* rolling least and the *Normandie* the most.

Now we unhesitatingly affirm that there is nothing to justify the conclusion that these gentlemen have come to, viz., that the greater rolling was caused by the greater metacentric height, but the reverse.

We accept the statement as to the performances of these ships as given by Sir S. Robinson.

1st. He says "that the *Solferino* is superior to all the ships of both squadrons," but this includes the *Achilles*, yet her metacentric height is only 3·1 feet, while that of the *Solferino* is 4·5 feet; therefore the theory breaks down; nor is this all, for the *Solferino* is nearly one-third smaller, *i.e.*, of 2600 tons less displacement, than *Achilles*, and, therefore, might reasonably be expected to roll more instead of less; and while they have the same extreme breadth, which is the disturbing dimension, the *Achilles* is 90 feet longer!

The decided superiority of *Solferino* must be due to the lower centre of gravity and greater metacentric height, and the weights being on her bottom tending to limit the arcs rolled through or to "check rolling."

Again he states "that *Achilles* is quite as good as the *Napoleon;*" what! not much better? Why the metacentric

height of *Napoleon* is 4·9 feet, that of the *Achilles* only 3·1 feet! The disturbing force of *Napoleon* may be represented by the cube of 55, that of the *Achilles* by the cube of 58; but then the latter is 147 feet longer and has only 4400 tons greater displacement! Moreover, the French ship not being an ironclad did not possess the alleged soporific of weighted sides. The theory here is doubly in fault. The true explanation is a low centre of gravity, great metacentric height, and weights on the floor.

Then the *Achilles* is said to be rather superior to the *Magenta*, when she ought to be very superior, being so much larger, that is, 90 feet longer and 2600 tons more displacement, with same breadth of beam, *i.e.*, an equal disturbing element. Here, also, the theory fails, and the explanation is a lower centre of gravity and weights on the floor!.

Also, *Achilles* is stated to be decidedly superior to the rest of the French squadron, excepting the *Tourville*. Why, the metacentric height of this ship is 5·3 feet, that of *Achilles* 3·1 feet; she is without the soporific of armour, is 140 feet shorter and 4000 tons less displacement than *Achilles*. Again the theory is doubly wrong.

Achilles is superior to the rest of the French squadron, *i.e.*, to the *Couronne*, the *Invincible*, and the *Normandie*. Really we are surprised that a direct comparison should have been made; she is 110 feet longer than *Couronne*, and 3300 tons more displacement, 118 feet longer than each of the other two, and 3600 tons greater displacement than one, and 3890 tons more than the other. That these did not roll very much more than her is due to their higher centre of gravity and greater metacentric height.

For, comparing the French ships amongst themselves in respect of their metacentric height, there is not even the show of reason; for, first, the two unarmoured ships must be struck out, as it is now admitted that they roll less than ironclads. The theory as to the contrary is now abandoned by Mr. Reed—reason and experience have re-obtained their sway.

The French ironclads take their places in the order of their size, the largest rolling least. The two first are sister ships, as also the two last, but of a lower class; the proportion of *breadth* to length in these last is greater than that of the other three, therefore they ought to roll more. The *Magenta* is 1100 greater displacement than the *Normandie*. Then the *Edgar** is said to be "inferior to the *Napoleon*,

* We take the metacentric height of *Edgar* to be the same as her sister ship, 4·6 feet.

Tourville, Solferino, Achilles, and *Magenta*." The inferiority cannot arise from her metacentric height, for it is equal to that of the *Napoleon*, but less than that of all the other French ships. Why should she be inferior to the *Napoleon* and *Tourville?*" Because being 3 feet wider, she is subject to greater disturbance. The other ships are all so much larger, especially the *Achilles*, that no comparison can be made; the *Achilles* is 140 feet longer, and 4000 greater displacement.

We take the metacentric height of *Black Prince* to be the same as that of her sister ship *Achilles*, that of *Hector* is 4·6, and *Black Prince* is a sister ship to *Achilles*, and ought not, as far as we are informed, to be different from her.

The *Defence* is said to be better than the *Hector* and *Prince Consort*. With the equal length the *Defence* has 2 feet less breadth than the *Hector*, and with 7 feet greater length she has 4 feet less breadth than the *Prince Consort*, therefore we should expect the latter to have greater motion than either of the others.

Then the *Black Prince*, with her smaller metacentric height, 3·1 feet, is admitted to be inferior to the *Solferino* and *Magenta* with their considerable height of metacentre, one 4·5 the other 5 feet.

We have before examined the facts offered in support of the theory as drawn from the comparison of the rolling of a number of English ironclads, and have shown they range themselves as respects their rolling exactly in the order of the proportion of breadth to length, the vessel with least proportionate breadth rolling least. The *Minotaur*, the largest and best ship, with a metacentric height of 3·8 feet, superior to the *Bellerophon* with only 3·2 feet of metacentric height. The *Pallas*, a ship of Mr. E. J. Reed's, the worst roller, her smaller metacentric height not checking the rolling so much as the great metacentric height of the *Prince Consort*, 6·1 feet.

It was natural for *Pallas* to roll; she had such a great proportionate breadth of plane of flotation.

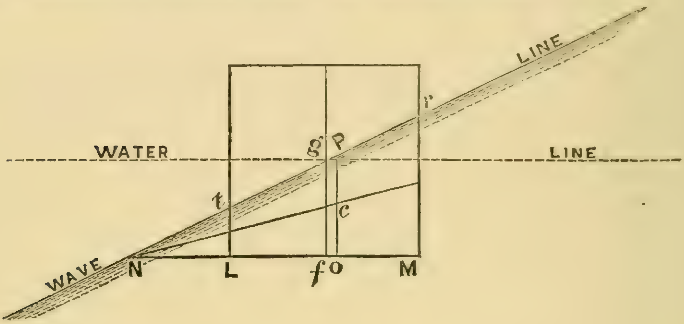
Raising weight did not cure, but the contrary; it made her rolling worse.

Obviously the theory that induced these gentlemen to damage a fleet of ships by reducing their stabilities has no foundation in experience, in reason, or in science.

And as this system might have been effectually tested in the course of an afternoon, not one of these unsafe ships should have been built.

Proof of Proposition stated page 478.

If, instead of supposing the floating body to be inclined at any given angle in smooth water, we suppose the surface of the wave to be inclined to the mean surface at an equal angle, it will be evident that the reasoning with reference to the pressures, pages 73 and 74, will hold good.



Take—

$$Mr = b,\ Lt = a,\ NM = c,\ NL = d,\ NO = X.$$

Take the usual formula for the centre of gravity, and integrating between the limits c and d—

$$X = \frac{\int xy\delta x}{\int y\delta x}$$

$$y = \frac{b}{c}x$$

$$\int xy\delta x = \frac{b}{c}\int x^2\delta x = \frac{b}{c}\frac{x^3}{3}$$

$$\int y\delta x = \frac{b}{c}\frac{x^2}{2}$$

$$\frac{\int_d^c xy\delta x}{\int_d^c y\delta x} = \frac{h}{3}\frac{c^3 - d^3}{c^2 - d^2} =$$

$$\frac{2}{3}\frac{c^2 + cd + d^2}{c + d}$$

$c + d = 24 \qquad 3(c+d) = 72$

$c^2 + cd + d^2$

$324 + 108 + 36 = 468$

$2(c^2 + cd + d^2) = 936$

$$X = \frac{936}{72} = 13.$$

Formula for the equilibrated lever $\frac{b^3}{12S} - a.$

Centre of gravity of the system being above the centre of displacement.

In this formula, b = the line of intersection of the cross section of the floating body when upright with the surface of the water; S, the figure of displacement when the body is upright; a, the distance between the centres of gravity and displacement when the body is upright.

In this case that of a block 12 feet square as described, page 72, &c.—

$$b = 12 \text{ and } S = 6 \times 12.$$

$$\frac{b^3}{12^2 \times 6} = 0{\cdot}5$$

$$op = \frac{13}{2} = 6{\cdot}5.$$

$$oc = \frac{op}{2} = 3{\cdot}25.$$

g being the centre of gravity of the floating block $f\,g$, evidently = 6.

$$a = b - 325 = 275$$

$$\frac{b^3}{12S} - a = 0{\cdot}5 - 2{\cdot}75 = -2{\cdot}25.$$

The stability being negative in this case; but as the centre of gravity is lowered, a is diminished till it becomes equal to $\frac{b^3}{12S}$, when the stability is that of indifference; when a is less than $\frac{b}{12S}$, the stability becomes positive and the righting lever increases, while the upsetting lever remains constant; the conditions in other respects remaining unaltered.

III. THE LUNAR ATMOSPHERE AND ITS INFLUENCE ON LUNAR QUESTIONS.

By Edmund Neison, F.R.A.S., &c.

PERHAPS no question in connection with the condition of the moon's surface has excited so much attention as the existence, or not, of a definite lunar atmosphere; and it must be conceded that it possesses the highest importance, when considered in its relation to the many problems of great interest to be decided in connection with the present and past history of our satellite.

Curiously enough, although the most diligent and expert selenographers, with hardly, perhaps, but one exception, have long recognised both direct and indirect evidences of the probable existence of an atmosphere, yet among astronomers in general it has been usually considered as definitely established that the moon is to all intents a perfectly airless globe. For even if it has been granted that it may possess, perhaps, the merest trifling trace of gaseous envelope, it has been considered established, for a certainty, that this must be far less dense than the most perfect vacuum that has been artificially produced by an air-pump.

There does not appear to exist, however, sufficient grounds for this widely entertained belief in the non-existence of a lunar atmosphere having been proved; for apart from the necessity, not observed in this case, of cautiously accepting positive conclusions based entirely on purely negative results, the subject has never been examined in a sufficiently complete and accurate manner. In consequence, many of the criteria that have been regarded as demonstrating the non-existence of a proper lunar atmosphere should never have been taken into consideration as applicable. Most of these negative results, which have been relied on as showing the absence of any gaseous envelope to our satellite, are only applicable to an atmosphere similar in density to our own. But such are the favourable conditions prevailing upon the terrestrial surface that if an atmosphere whose mass and that of the planets were in a constant ratio were to exist on every planet, the earth's would be far the densest of any, excepting the giant of the family Jupiter. But on the moon the most unfavourable conditions prevail for the density of atmosphere; and under the conditions supposed, and these conditions the most probable imaginable, then the lunar atmosphere would possess at the surface a density only

about one-fiftieth of the density at the terrestrial surface. Notwithstanding this, however, as before stated, the ratio between the masses of the atmosphere and planet would be the same as on the earth. All criteria directed, therefore, against an atmosphere similar in density to our own, are quite inapplicable to the moon's; and an examination of any of the works treating on the question will show how many are of this class.

Further examination will also show that we have probably to deal with even a less density than some one-fiftieth of the earth's. The moon's mean density being only three-fifths of the earth's, the moon's surface is, in proportion to the lunar mass, over six times greater than on the earth. Accordingly the atmosphere would be subjected to the action of a surface more than six times as great as has been on the earth. There can be little question but that at one time the terrestrial surface must have exerted a powerful action on our primitive atmosphere, and have absorbed and locked up much of what constituted our earliest atmosphere, for on no other conditions can be explained the known nature of the terrestrial surface. But as the moon's primitive atmosphere must have been exposed to the action of more than six times the extent of surface, it would appear not improbable that her atmosphere must have lost much more in proportion than the earth. Even, therefore, if both planets originally possessed atmospheres whose masses, and those of the planets, were in similar ratio, the moon's, thus exposed to a far greater absorbing surface, must in the end have lost the most; and so the residue left possess a smaller ratio to the mass of the planet than on the earth. We can easily see, therefore, how—by the action of this proportionately six times greater surface—it is possible for the moon's atmosphere to have been reduced to one-sixth the ratio to the mass of the planet that we find on our terrestrial surface. It is true that this would be a very difficult question to in any way prove, or even to maintain in its integrity, but this is immaterial for our present purpose; and it may well be admissible that, even had the planets started with the same proportional atmosphere, that of the moon may have been reduced to one-sixth. As the lunar atmosphere, under equal conditions, has been shown to be only one-fiftieth of the terrestrial density, reduced to one-sixth it would possess only one three-hundredth, or possess a density expressible on the earth as one-tenth of an inch, or some $2\frac{1}{2}$ millimetres. Now such an atmosphere is possible.

But such an atmosphere is not small; the only thing

small about it is its density, as compared with the very dense atmosphere of the earth, while its real magnitude and mass is immense ; and the mass above equal portions of the surface is not so much smaller than on the earth.

To ascertain what atmosphere is possible on the moon, it is necessary that we should express, by means of a few simple equations, the conditions any atmosphere would have. As, however, the derivation of these equations from the known physical laws, with respect to gases, would involve the application of more complex mathematics, it will be dispensed with. Throughout, it may be observed, it is assumed that the moon's atmosphere would be essentially similar in composition to the earth's, with perhaps a much larger percentage of carbonic anhydride, which is to be expected.

Let a = the radius of the moon in miles;

x = any height in miles above the surface ;

δ_0, p_0, t_0 = the density, pressure, and temperature of the atmosphere at the lunar surface, and δ, p, t the same at height x.

And put for the constants involved—

$\alpha = 0{\cdot}000294$, multiplied by the density of the lunar atmosphere in terms of the standard terrestrial density of air;

$\epsilon = 0{\cdot}003665$, the expansion of gases for 1° C. ;

l_0 = the height in miles of a column of the lunar atmosphere of density δ_0, at temperature t_0 that would—under the action of gravity at the moon's surface—exert a pressure of p_0. From this—

$$l_0 = 32{\cdot}6837\,(1+\epsilon\, t_0) \text{ miles.}$$

Finally, for brevity, write θ for $\dfrac{a}{l_0}$.

Then the equations expressing the physical condition of the lunar atmosphere may be written—

$$x = l_0\left\{(1-f)\,u + 2f\,(1-e^{-u})\right\} \quad . \quad . \quad . \quad (1)$$

Where e is the base of the natural logarithms—

$$\delta = \delta_0\, e^{-u} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

$$p = p_0\, e^{-u}\,(1-f+f e^{-u}) \quad . \; . \; . \; . \; . \; . \quad (3)$$

$$\frac{1+\epsilon\, t}{1+\epsilon\, t_0} = 1-f\,(1-e^{-u}) \quad . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

And lastly, for the horizontal refraction r_0, expressed in seconds of arc, undergone by a ray of light in passing through the atmosphere—

$$r_0 = \frac{a(1+a)}{\sin 1''}\sqrt{\frac{\theta\pi}{2}}\left\{1-(f-a\theta)(\sqrt{2}-1)+f(2-\sqrt{2})\right\} \quad . \quad (5)$$

where f is the function on which the rate of decrease of temperature depends.

Were the temperature of an atmosphere constant throughout, the density and pressure would, in accordance with Mariotte's law, diminish together in geometrical proportion with increase of altitude. The temperature is, however, not constant, but varies inversely as the altitude; so it becomes necessary to assume some law for this decrease, and this will evidently depend primarily upon the supposed temperature of space. It has been generally assumed, by those who have lately written on this lunar subject, that the temperature of space is near the absolute *zero* of heat,—in contradistinction to Fourier and Hopkins, who regarded it as but little below *zero*, which is preferable. Whatever value may be adopted, the corresponding value of the function f is given by the equation—

$$f = 1 - \frac{1+\varepsilon t}{1+\varepsilon t_0}, \quad . \quad . \quad . \quad (6)$$

as the temperature of space would not be reached until a considerable distance from the moon.

These equations, it is seen, involve the temperature of the lunar surface, and from Lord Rosse's determinations it appears that the moon's surface may reach 200° C. For the minimum value the temperature reaches during the long lunar night very different values have been ascribed, and it is evident that much may depend on the temperature of space assumed. It has, moreover, been generally thought that any lunar atmosphere would, from its extremely small density, exert little influence in retarding the radiation of heat from the moon; but it has been overlooked that the thickness of the envelope of atmosphere, as well as its density, must be considered. Now the thickness of the lunar atmosphere is for this purpose many times greater than the earth's, and this counterbalances its tenuity; accordingly the retardation of radiation would not be much less than on the earth. As during our long terrestrial polar nights, from six to ten times the length of the lunar night, the mean temperature is not usually many degrees below zero, it will therefore be probable that the temperature of

the moon's face will not fall either much below zero, for there is more heat to be lost than on the earth. It is, moreover, to be remembered, that the proportion of carbonic anhydride in the moon's atmosphere would be probably much greater than in the earth's, and this powerfully retards the radiation of heat. From these considerations, then, we have at sunrise the moon's surface at any position near the equator, the temperature about -20°, which will increase until a little after full moon, when it will have risen to about 200°, and will then slowly decrease, until at sunset it will be about 60°. During the lunar night the temperature will fall slowly until a few days before sunrise, when it may become as low as -30°, rising again to about -20° at sunrise. But it is evident that as the temperature is solely due to the solar rays, these must be the extreme results; and that for any place away from the lunar equator the climate will be much milder, never rising to so high a temperature, as the sun will not be in its zenith, nor ever falling so low.* This circumstance has usually been overlooked.

The temperature of the lunar surface having been therefore approximately determined, the equations can be at once applied to find the conditions of the greatest lunar atmosphere possible under known circumstances.

An examination of all the methods that have been applied to detect a lunar atmosphere shows that all the most delicate —and, in fact, only applicable ones, to such an atmosphere as we have seen would probably exist—are based upon the refraction of light by the atmosphere. And the condition under which this refraction is most marked is that which requires attention, namely, the horizontal refraction. And even of these selected methods, the most delicate and only thoroughly reliable one is that based on the retardation of the occultation of a star, at the dark limb of the moon, by the refraction caused by the light traversing the atmosphere. For when the diameter of the moon is known, the exact

* The values in the text are arbitrary, but have been arrived at from theoretical consideration in conjunction with Lord Rosse' determination, and can be expressed by the following equation, which gives the theoretical results for the maximum and minimum taken. Assume ξ is the solar altitude at any spot, and increases from 0 to its maximum, and then falls to 0, at which it remains while the sun is beneath the horizon. Put ζ equal to 300 degrees at sunrise, and rising uniformly by 2π during each lunation; then if λ be the latitude of the spot we have roughly—

$$t_0 = 170^{\circ}(1 - A\cos\xi + B\sin\zeta - C\sin\lambda).$$

It has been taken that $A = 0{\cdot}9$. B, which varies as the cosine of the latitude, is $0{\cdot}28$ at the equator, and $C = 0{\cdot}08$, which values but represent the theoretical probabilities.

time of the disappearance of the star, were there no atmosphere, is easily calculated; and if, therefore, it is found that the observed time of occultation is later than the calculated, then a lunar atmosphere—of a density that would cause this retardation—appears certain.

Unfortunately the moon's diameter is not known with exact accuracy for certain, although it is so within very small limits; and this somewhat complicates matters. Sir George Airy has determined, from transit circle observations, what is regarded as a very accurate value for the moon's telescopic semi-diameter. This value has been considered very slightly too great, owing to the effect of irradiation at the lunar limb caused by the contrast between the bright moon and the sky; although a careful examination of the observation of the lunar diameter shows that this is questionable, and renders it doubtful whether the variation found with different instruments is not due solely to the spurious telescopic disc.

Observations made with the Great Equatorial, at Greenwich, during the solar eclipses of July, 1860, and December, 1870, enable this to a great extent to be rectified, by showing the minimum value for the lunar semi-diameter to be some 15′ 34·00″—a value in agreement with the observations of Greenwich and Oxford. For it is apparent that during a solar eclipse there is an absolute reversal of the effects of irradiation, as they must then—to a much greater extent—diminish the apparent diameter of the moon than under ordinary conditions they can ever increase it,—as the contrast between the dark moon and the extremely bright disc of the sun is vastly greater than the contrast between the comparatively feeble light of the moon and the sky. Moreover, as the point to be determined in observation during a solar eclipse is where darkness ceases, we have absolutely the fullest action of irradiation, together with the effect of a maximum spurious telescopic disc; while under ordinary conditions of observing the moon's limb with the transit circle a smaller spurious disc comes into play, and it is questionable whether the effect of irradiation on these observations has not been over-estimated. In, therefore, observations of a solar eclipse the moon's apparent diameter is reduced to a much greater extent than under ordinary conditions it can ever be increased; and we have, accordingly, the value for the moon's minimum diameter before stated.

The minimum lunar semi-diameter may be considered as 15′ 34·00″, although it is possible that the true diameter is

somewhat larger, as indeed is indicated by the observation made during the eclipse of 1870, the most favourable one of the two. It remains now to see whether, with this lunar semi-diameter, the observed occultation of stars by the moon's dark limb indicate the presence of any lunar atmosphere; for the occultations at the bright limb are very inferior, and the re-appearances at both limbs are almost valueless.

From a large number of observations of occultations of stars by the moon—made between 1850 and 1873 when very satisfactory lunar places had been obtainable, and observed at Greenwich, Oxford, and Cambridge—it has been shown* by the author that the lunar semi-diameter derived from the occultations at the dark limb of the moon is less than the adopted minimum semi-diameter by over two seconds of arc, corresponding usually to from five to ten seconds of time. The retardation of an occultation being less than twice the horizontal refraction,† this would indicate the existence of an atmosphere exerting a horizontal refraction of a little over one second of arc; and the possibility of its existence appears beyond question, for no other method of detecting an atmosphere approaches this in delicacy.‡

The equations on which the physical conditions of the moon's atmosphere depend may at once be employed to find the density of a lunar atmosphere whose horizontal refraction will be a little greater than one second at the temperature of about zero. By putting the density of the moon's atmosphere at about one three-hundredth of the earth's, it will be found to satisfy this condition, keeping the value of f moderate. It will also be found that, as the average temperature of the moon's bright limb is high, the refraction exerted by the atmosphere is only about one-half of that at the dark limb; and, if allowance is made for the effects of the spurious telescopic disc at whose edge the star will disappear usually, it will be found that the observations of occultations at the bright limb give agreeing results.

The great difference in the density of atmosphere arrived at for somewhat similar retardation of occultation—for Sir George Airy has long seen the possibility of an atmo-

* Monthly Notices of the Royal Astronomical Society, vol. xxxiv., p. 356.

† In the Monthly Notices, vol. xxxiv, p. 20, the late Sir John Herschel's value has been taken, but it has since been found that the above is correct. This requires the suppression of the first sentence of the bottom paragraph of that page.

‡ That further discoveries and observations may possibly modify this conclusion is of course unquestionable, but this constitutes no reason for altering any deduction to be drawn from the circumstances as they exist at present.

sphere being the cause of this—arises from a very simple cause. From merely looking at the question in a general manner it has been always considered that for a similar surface density the refraction on the moon and earth would be alike; but a mere inspection of the complete differential equation to the refraction of light through an atmosphere shows that the refraction for equal surface densities varies approximately for small refraction as the square root of the radius of the planet and the action of gravity on the surface, and on the moon both, are much less than on the earth. On the moon accordingly, for equal surface densities, the refraction is only about one-fifth of what it would be on the terrestrial surface, and, this circumstance having been overlooked, the density of the atmosphere has been greatly underestimated.

It would be useless to travel through the numerous methods that have been referred to as showing the non-existence of any lunar atmosphere, not of much greater tenuity than the most perfect vacuum of an air-pump, for they are not applicable to such an atmosphere, although it could hardly be called even a vacuum, much less a perfect air-pump vacuum, but a few general remarks will suffice. The effect of the atmosphere on the phenomena presented by the moon would be imperceptible in all except the single case of the retardation of occultations, for the amount of refraction is insufficient to in any way distort or alter the appearance of a star or planet seen through it, and, similarly, any effect it might exert upon the spectroscopic observations of our satellite would be marked by the greater action of the terrestrial atmosphere. The sole action it would exert during a solar eclipse would obviously be to simply augment the solar diameter to a very slight extent, but, as this latter is not known with accuracy, this would be undetectable; neither would any new lines be revealed by the spectroscope in observations like Mr. Stone's,* for there would be no new substance to give them, as would be indeed very improbable. Finally, it may be observed that the rays, after refraction through the atmosphere, would not be convergent like after passing through a lens, for, the refraction diminishing with the height, rays from different altitudes would be divergent. This atmosphere, which at first sight might appear so rare as to be unworthy of notice, is not so in reality, for it is of much greater proportionate dimensions than the earth's, which it even exceeds in actual volume, counterbalancing its

* Monthly Notices of the Royal Astronomical Society, June, 1874.

less density. Consequently, the amount of atmosphere acting on the moon's surface is not much smaller than on the earth, and, as far as a single square mile of surface is concerned must be estimated by millions of tons. Moreover, from these considerations as to the horizontal refraction exerted by a lunar atmosphere, it is evident that, were its density a little less than one-thousandth of the terrestrial density, the retardation at the limb during occultations would no longer be detectable from its being smaller than the probable errors of the method, and yet it cannot be questioned but that this atmosphere would still exert a most marked influence on the physical condition of the lunar surface.

There are many selenographical problems dependent on the question of the existence or not of a lunar atmosphere, and prominent amongst them stands the absorbing subject, so often debated, of lunar changes. Though the configuration of the surface of the earth is undergoing a never-ceasing change from the action not only of volcanic forces, but also of the various atmospheric and aqueous causes, were it to be regarded from the moon, it is doubtful whether during the last century, with all our telescopic aids, any unmistakable alteration in the formations constituting it, or in its appearance even, apart from changes of tint, could be detected. Similarly on the moon there may be even more energetic action in constant work, without its making such vast changes in the appearance of our satellite as to direct our attention unmistakably to the circumstance of lunar, volcanic, and other energies being still in active progression. For such is our imperfect knowledge of the details of the configuration of the surface of the moon, that, were at intervals of a few years new volcanic cones, craterlets, and hills to come into existence, it is improbable in the extreme that they would be recognised as new formations instead of new objects detected.

Suppose the new manifestation of lunar volcanic energy took the form of some of those volcanic cones, as occur on both the earth and moon—a steepish hill, some hundred yards in diameter and hundred feet high, with a deep mouth of no great dimensions ; or else, perhaps, gave rise to a new ridge of mountains, such as are so common on the moon, some miles long and a mile broad, covered, perhaps, with small crater cones not exceeding some hundred yards in dimensions—there is not one ten-thousandth of the moon's surface where these new formations would possess the slightest probability of being taken for the result of the action of present lunar volcanic energy. Or were, from some vast

convulsion, a magnificent lunar crater to be formed, and cover the surface around with streams of lava, with a mouth two thousand yards in diameter and a thousand feet deep, its discovery would be improbable—nay, unless it occurred close to some few small localities, it is almost certain that it would not be detected as a new formation. Assuming, however, that one of these new formations were to appear, or some craterlet a thousand feet deep and several miles in diameter collapse into ruins, and granting one or two of the very few astronomers who study the moon's surface had for years sufficiently studied the small portion of surface where it occurred as to be sure it was a new formation, it would then be found impossible to prove it to other astronomers.

The actual effects of libration on the moon, in ever presenting the objects on its surface in new lights and at different angles, makes the lunar formations always variable in appearance to some extent ; while the general opinion of the powers of libration to change the appearance, visibility, and form of lunar objects, even near the lunar centre, is such that the most marked changes of appearance are ascribed to its effects. It would therefore require a most striking and wonderfully marked difference in the appearance of a large lunar formation to occur, before the existence of lunar volcanic, or other inherent energies would be admitted as demonstrated, or perhaps probable. There is, in fact, a certain amount of basis for this reluctance to admit the existence of lunar changes, for certainly the difficulty in the study of the details of the moon's surface is such as to render great caution necessary ; but it must not be overlooked, as is commonly done, that this in itself prevents the absence of any proved case of change in the lunar formations being accepted as a complete demonstration of the cessation of the action of all volcanic and similar forces on the surface of the moon.

Those astronomers who have made the details of the surface of the moon their study, are well acquainted with the almost absolute want of knowledge of the real minute details of the surface, and accordingly have long been convinced, with perhaps the exception of Beer and Mädler, that we are without any evidence of the constancy of the configuration of the moon's surface, with one exception, and this bar to the possibility of the moon's volcanic and other sub-lunarian energies being still actively at work was the supposed absence of an atmosphere. Volcanic action and no atmosphere seems inconceivable, as De la Rue has long remarked, but, grant the existence of the lunar atmosphere,

and at present there is no evidence that the moon's volcanic energies are not tenfold more active than on the earth, and the importance of the determination of the probability of a lunar atmosphere is evident.

There is another question which is completely controlled by that of a lunar atmosphere. Most selenographers, and even the most cautious, Beer and Mädler, have recognised on the moon indications of the existence of some form of vegetable life, but, again, vegetation and no atmosphere is inconceivable; waiving the question of moisture as carrying us into too large a field, and granting the existence of an atmosphere, and most interesting problems are opened. For the researches of M. Paul Bèrt have shown that vegetable life is possible under atmospheric pressures only one-twentieth of the normal terrestrial one, and not so much more than is possible on the moon; considering, therefore, the wonderful adaptability to circumstances of vegetable life, who can say it is impossible at a pressure less than this? The moon's temperature need be no bar, for, although a fatal objection near the equator, it is no longer so nearer the poles.

Similarly with the many more recondite problems that the study of the moon's surface has given rise to—the streaks and rays of the full moon, the variability of the markings, floor, and craterlets of Plato, the white cloud-like coverings of Linne and γ Posidonius, the difference in the polarisation of the light reflected from the plains and mountains observed by Secchi, in their photographic power found by De la Rue, in their colour by Birt, in their visibility by Schmidt—all involve in their consideration the question as to the existence of a lunar atmosphere. It must be at once admitted that the existence of a lunar atmosphere facilitating the probable explanation of these phenomena proves nothing; for the circumstance, that granted a basis and all can be explained, is no demonstration of the truth of that basis. But neither must it be assumed, as is so often done, that the possibility of accounting for phenomena on other grounds shows the proposed explanation to be probably wrong; the nature of the two must always be considered. For example, it is possible to explain the photometric results of Zöllner as to the brightness of the moon by assuming either that the lunar surface is covered by a multitude of very minute steep conical hills, each of a definite slope, and arranged in a complex manner round two common foci, or else that it is covered with extremely small regular furrows of uniform angle, running from pole to pole at proper distances; and these hypotheses explain most satisfactorily all Zöllner's

observations; but they cannot be considered as disturbing the explanation that the cause of the phenomena in question is simply the known irregularities on the surface, which also accounts for the observations, although not so well. It is, however, too generally considered that the possibility of two explanations of one circumstance weakens the probability of both.

Competent dealing with the problem of the physical condition of the surface of the moon requires a special knowledge of the minuter details of the lunar surface, rarely met with in combination with a proper acquaintance with the more theoretical elements involved in the subject, and, in consequence, questions have often been considered as standing in a different position to their real nature. A mere general familiarity with the principal configuration of the moon's surface and the phenomena to be seen is not sufficient in the hands of the most able theorist for the proper consideration of the complex problems involved, and it is not strange if the resulting conclusions are often not all that could be desired.

The study of the details of the moon's surface, until very recently long left in the hands of a very few indefatigable astronomers, from the interest inseparable from it is now attracting the more general attention it demands, and gives promise of before long placing us in a more satisfactory position with regard to the nature of the lunar details. The admirable work of Beer and Mädler, so far from being exhaustive as was long thought, brought us to only the threshold: Schmidt's forty years' work, gigantic as are the results, only reaches the minutiæ, and will require years of revision; while the real character of the objects now known to exist merely, is nearly untouched ground, and it is there where some of the greatest difficulties will be felt, for to evolve a theory as to their source is far different from detecting their true origin. But there is promise ere many years are over that we may be in possession of a sufficiency of exact knowledge of the features presented by the moon to enable some more satisfactory conclusions to be drawn as to the events of the past history, present condition, and immediate future of our satellite.

IV. BERYLS AND EMERALDS.

By Professor A. H. Church, M.A., &c.

ALTHOUGH to the mineralogist the stones known as aquamarine, beryl, and emerald, constitute but one species, there are decided differences of optical properties, as well as minute differences of chemical composition, between them. Under the name "beryl" minerals of considerable variety of aspect are thus included; some specimens being nearly opaque and brownish-yellow, others being perfectly transparent and colourless, while others, again, are beautifully tinted with a pale sea-green hue. But other colours also occur, such as yellowish-green, pale blue, and even lilac. Still it is in the emerald, once thought to be a distinct mineral species, that the richest and most highly appreciated colour is found. The pure green of this gem has provoked several enquiries as to its nature, and, quite recently, Mr. Greville Williams* has taken up the enquiry, and published the first portion of his results. In presenting an account of this research to our readers, we will also give some facts derived from other sources concerning the transparent beryl and the emerald.

This mineral species occurs only in crystals, while such divergences from constancy of composition as it exhibits affect scarcely more than 2 parts out of 100 in any specimen. It is essentially a silicate of alumina and glucina, but it always contains a little iron, while chromium seems a constant ingredient of the true emerald, though occurring as a trace merely, not amounting to a half per cent when calculated as sesquioxide. Other metals have been recognised in some varieties of the beryl by different analysts, and the list will include calcium, magnesium, manganese, and tantalum.

The specific gravity of transparent pale beryls is, as nearly as may be, 2·7, so that it is decidedly denser than rock crystal. This fact may be beautifully illustrated by means of a solution of mercuric iodide in potassium iodide.† Such a liquid may be readily obtained with a sp. gr. = 3·0, but a solution not denser than 2·67 or 2·68, nor lighter than 2·665, is required in the particular case in point. In such a liquid a piece of flawless rock crystal floats, for its specific

* Proc. Roy. Soc., No. 145, 1873.

† Recommended by Mr. E. Sonstadt for such experiments, in the "Chemical News," vol. xxix., p. 127.

gravity is but 2·652 at the most, while all beryls, when free from any kind of imperfections, such as cavities, fissures, &c., appear to range in density from 2·69 to 2·705, and consequently sink. The same is true of the emerald when perfect, but the general presence of internal flaws in this stone, even though they may be very minute, lowers its specific gravity decidedly, and may even reduce it to that of rock crystal. It should be stated here that the specific gravity of beryl is not altered by any temperature short of that necessary to effect the fusion of the stone, and thereby to change its crystalline structure to the condition of a glass.

A word concerning the hardness of beryl will suffice. Pure, transparent, and colourless or pale specimens are harder than quartz, varying from 7·5 to nearly 8; yet the best emeralds—though they scratch quartz, and of course all kinds of glass and paste as well—are slightly softer than the paler varieties of the species, such as the aquamarine.

We now come to the question of the colour of the emerald, a question which has been lately studied with particular care by Mr. Greville Williams. The green hue of this gem is of a different quality from that of the precious beryl and the aquamarine,—indeed it closely approaches, in really good specimens, to the pure green of the solar spectrum. The colour is, also, far more permanent than that of some beryls, especially those of a yellow or greenish-yellow hue, which become bluish, or even pale blue, when heated before an ordinary gas blowpipe jet, long before they show any signs of incipient fusion. The emerald, on the other hand, when heated alone before the oxyhydrogen blowpipe, bears a bright red heat without loss of colour, and even when it begins to fuse the edges only become colourless and opaque, the centre remaining green. Even when fused, emeralds are kept at the maximum temperature attainable by means of the oxyhydrogen blowpipe, they retain an opalescent dull green hue for some time, and it is only after prolonged heating that they become at last transparent, and almost free from colour. Greville Williams has also found that the addition of chromium sesquioxide to a fused emerald bead which has become colourless produces only a dull green colour, or, at the best, a colour decidedly inferior to that of the emerald. Cobalt oxide gives a fine blue tint when similarly added to an artificial mixture having the composition of the natural beryl or emerald, and this blue colour is not altered by the high temperature of the oxyhydrogen flame. With such artificial beryl composition

other coloured glasses may be made, one of the most interesting of these being that containing didymium oxide, which imparts a lively pink colour to the fused bead. Moreover, this beryl glass shows the black absorption spectrum of didymium in a very perfect manner. The artificial beryl-glasses were made by taking the essential constituents of the mineral in the percentage proportions here given:—

Silica	67·5
Alumina	18·5
Glucina	14·0
	100·0

By the fusion of these ingredients together a beryl-glass is obtained which is practically identical in physical and chemical characters with that resulting from the fusion of the native emerald or beryl. In each case the hardness is reduced below 7 (that of quartz), and the specific gravity is only 2·42 instead of 2·7. These are signs that the compound has passed into the vitreous condition, and that it is destitute of crystalline structure. Though really identical in chemical composition with the native beryl, these beads are nothing more than "beryl-glasses," and can never in such a condition compete with the true native stones. Even were the exact tint of colour to be achieved, and if the hardness and specific gravity of the artificial glasses were much increased, still there would remain one fatal inferiority to the true emerald. For an emerald is dichroic, exhibiting, when viewed along the principal axis of the crystal, a green hue decidedly dashed with yellow, while when viewed in any direction transverse to this it shows a green colour verging a little towards blue. The instrument known as Haidinger's "dichroiscope" enables these two pencils, of differently coloured (and differently polarised) rays, to be seen alongside of each other if a beam of white light be transmitted through the crystal along one of the secondary axes. Now the crystalline structure upon which this dichroism depends has not yet been imitated in the artificial emerald, and in consequence the play or fluctuation of three hues—the normal emerald-green, the yellowish-green, and the bluish-green—of the natural crystal must be wanting in any vitreous imitation of it. This dichroism of the best emeralds is often seen when a number of small gems have been cut from a single crystal of apparently uniform colour; some of the polished pieces, having been cut in a different direction with regard to the axes of the crystal, will show decided

variation in hue. The dichroism of other coloured beryls, as well as the emerald, is often well marked.

One of the main objects for which Greville Williams's research was undertaken was the settlement of the relation between the so-called organic matter found in the emerald and the colour of the stone. By an ingenious arrangement of apparatus, and by the use of the chromic acid method of burning the carbon and hydrogen present, this chemist has shown that colourless beryls sometimes contain carbon and hydrogen, just as the emerald does; and he has demonstrated that the carbon does not exist as a carbonate, though it may possibly occur in the form of microscopic diamonds. We are promised a further contribution to the chemistry of the emerald, with especial reference to the modes of separating and estimating the rare earth, glucina, which it—in common with the chrysoberyl, another mineral used in ornamental jewellery—also contains. The chemist Lœwy, who believed the colour of the emerald to be due to organic matter, detected the earth glucina, to the extent of $\frac{1}{2}$ per cent, in the calcareous concretions which are associated with the matrix in which the mineral is found at Muzo, New Granada: pyrites, and the rare carbonate of the cerium metals known by the name of Parisite, also occur with the emerald. It would be as well to examine the Brazilian emerald for cerium and its allies.

The emerald is more prized now than formerly, as much as £40 having been given for a perfect emerald of 1 carat (0·20541 of a gramme, or 3·17 grains). An emerald of 1 carat, owing to its low specific gravity, is of course much larger than a sapphire of the same weight, and about one-third larger than a diamond. Large emeralds are seldom of uniform and deep colour, and hardly ever free from numerous cavities and flaws. In the Duke of Devonshire's collection there is a splendid crystal, about 2 inches in each direction, and weighing nearly 9 ounces troy, but it is much flawed. In the Green Vaults of Dresden there are some fine stones, while there are also numerous specimens in the Brazilian, Turkish, Portuguese, Austrian, Persian, and Papal Treasuries. Many of these have, however, been disfigured by perforations or inartistic engravings, and being in many cases merely rounded and polished, "*en cabochon*," are not capable of showing to full advantage those optical properties of dispersion, refraction, dichroism, &c., which may be developed by the judicious cutting of this gem. In the South Kensington Museum, among the rings of the Townshend collection, is a superb emerald, *step-cut*, and showing the

characteristics of this stone in an eminent degree. The small so-called emerald of the Hope collection, in the same Museum, is nothing more than a piece of green glass. The true emerald is not well adapted for the purposes of the cameo and intaglio cutter, and was seldom engraved in Europe, although some examples exist,—as the Duke of Buccleuch's fine Medusa's head on emerald (a Florentine work of the seventeenth century). In Persia, India, &c., on the other hand, engraved or at least inscribed emeralds are not uncommon. But the transparent beryl and particularly the aquamarine have frequently been engraved with great skill and beautiful effect, both in classic and later times. Several good specimens of this kind are preserved in different public and private collections of gems, both in England and on the Continent. In the Bibliothèque Nationale of Paris there is a fine cameo of Julia, the daughter of Titus: the British Museum possesses some beautiful engravings on the same material, and there are others in the Museums of Florence, Naples, &c.

Some perfectly transparent and flawless aquamarines have been found of enormous size. One Brazilian stone weighs 225 troy ounces, and is as big as the head of a calf! The writer of this notice has seen one in London, of a rough spherical form, of perfect purity, though pale in tint, and nearly 4 inches in diameter. Mr. Hope possesses, in the hilt of Murat's sword, an aquamarine of remarkable size: there is also a fine richly-coloured stone amongst the Crown jewels in the Tower. In many private collections and in public galleries the different varieties of the beryl may be found in abundance: there is a good series of specimen crystals in the Mineralogical Department of the British Museum.

V. ON THE CURVED APPEARANCE OF COMETS' TAILS.

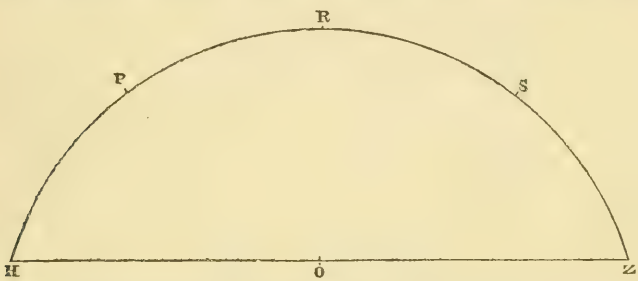
By Lieut.-Col. DRAYSON, R.A., F.R.A.S.

IN our paper on the Pole Star and Pointers in this Journal, we called attention to the fact that what would appear as a straight line to an observer in one part of the world, would appear as a curve or arch in the heavens to an observer on another part of the earth's surface.

As an illustration of this law, we called particular attention

to the trace of the equinoctial on the sphere of the heavens, and pointed out how to an observer in middle latitudes the equinoctial would appear like an arch, cutting the east and west points of the horizon, and rising on the meridian to an altitude equal to the colatitude of the station on which the observer is located, whilst to the individual at the equator the trace of the equinoctial would appear as a *straight* line, rising from the east and west points straight to his zenith. Again we showed that to an observer at either pole the equinoctial traced in the heavens would coincide with the horizon, and would again appear as a straight line. It was also shown that a great circle passing from the east and west points of the horizon and through the pole would, for that part above the horizon and visible, appear as an arch to an observer in middle latitudes.

We will now suppose that three stars or any three celestial bodies (distant from the earth many million miles)



were located on the equinoctial; that to an observer at the earth's equator these three stars, which we will call P, R, S, were so situated that the star R was at the zenith, the star P east, and the star S west of the observer, and both 45° above the horizon. These three stars being coincident with the equinoctial would appear in a straight line to the observer at the equator, and the line joining P R S would also appear as a straight line.

To an observer in, say, 51° north latitude, the equinoctial appears as an arch or curve in the heavens and not as a straight line, and all portions of the equinoctial appear as portions of an arch or curve, and therefore the line joining the three stars, P R S, would appear as an arch or curve to the observer in lat. 51°, though it would appear a straight line to an observer at the equator. The three stars on the equinoctial would appear curved to an observer in lat. 51°, as shown in the diagram, where H is the east, Z the west points on the

horizon, H P R S Z a portion of the equinoctial, and P R S three stars on the equinoctial. The line P R S will appear as a curve or arch.

We will now suppose that S is the head or nucleus of a comet, and that the tail extends along the equinoctial and as far as P. The comet's tail would now reach from P to R and S, and would appear as a curved line or arch to an observer in 51° latitude and to observers in all middle latitudes, but as a straight line to an observer at the equator, or to one at the North Pole. Consequently the curving or bending of a comet's tail is in this case an appearance only, due to the position of the observer, and the same comet's tail may appear at the same instant to two observers, one in middle latitudes and the other at the equator, as a curve and a straight line.

When the comet's tail is extended in that part of the heavens near the North Pole, a quarter rotation of the earth will produce similar effects to the preceding. As an example, suppose the head of a comet west of the pole and the tail coincident with the great circle passing from the west point on the horizon through the pole, then the tail being coincident with this great circle would appear curved.

In six hours the comet would be on the meridian and below the pole, and the tail being coincident with this meridian, would now appear as a straight line, whereas before it appeared as a curve. Thus the curvature of a comet's tail is an appearance only, and is due to the same law as that which causes the pointers to sometimes appear to point more directly towards the pole star than they do at other times.

During the year 1861 we happened to be staying in Hampshire with a friend who was a close observer of Nature. We were walking in a wood a few minutes before sunset, when happening to look at the sky through an opening in the trees, we remarked a bright object larger than Venus at her greatest brilliancy. The object we at once saw was in the north, and we therefore knew that it was no star or planet which had so brilliant an appearance, and before the sunlight had sufficiently decreased to show a tail, we had decided that a remarkable comet must have come suddenly upon us.

As the sun descended below the horizon and darkness stole over the scene, the comet's tail became visible, and stretched from the nucleus, which was below the pole and near the northern horizon to far up above the zenith and over towards the south. By means of a pocket

sextant, which we had with us, we measured the length of the tail by measuring from the disc of the comet to those stars which could be seen within the tail. We found that 110° was the length of the tail, and we noted those stars which appeared to be in the centre line of the tail.

At about four o'clock in the morning our friend called us to come and look at the comet, which he announced had undergone a marvellous change.

When we first saw the comet and up to nearly midnight the tail appeared straight, and to point past the pole and up to the zenith; but at 4 A.M. the comet's tail had quite altered its shape, and, as our friend remarked, was more curved than a Turkish scimitar. Being aware at the time of those laws of curves and straight lines demonstrated in the preceding papers, we saw how good an example this comet afforded of the laws, for it was a practical illustration of a straight line becoming in appearance a curved line in consequence of six hours of diurnal rotation. We remarked that the comet's tail had not changed its position in the heavens as regards the fixed stars in the slightest degree, the very stars that were in the centre of the tail at 10 P.M. were also there at 4 AM.; and, in fact, we had the same problem exhibited in the heavens as we have endeavoured to demonstrate by diagrams in the preceding papers. The particular position occupied by the comet of 1861 was such that in six hours it changed the apparent form from a straight line to a curve, as seen by an observer in England, the comet's tail being directed from the northern horizon past the pole and zenith. If this comet instead of being so situated had its tail coincident with the equinoctial, it would never have appeared to us in England as a straight line, but always as a curve in the heavens during the whole time it was above the horizon. If, however, it had coincided with the equinoctial it would have appeared as a straight line to an observer at the equator of the earth, and never as a curve or arch. Thus, whether a comet's tail appears as a straight line or as a curve depends mainly on the position of the comet in the heavens and on the position of the observer on the earth. If a comet's tail were coincident with the equinoctial, and of, say, 80° length, it would appear to an observer in 45° N. lat. as a curve or arch. If this observer could be instantly transported to the equator, the comet would then appear as a straight line without any sign of curvature. If, again, the person were transported to 45° S. lat., the comet would again appear as a curve. A comet, therefore, that coincides with the equinoctial appears as a

curve or a straight line, according as the observer is at one or another part of the earth, whereas a comet whose tail passes through the pole appears as a straight line or curve, according as the tail is directed towards the zenith or towards the east or west. Thus rotation in the last case produces the same effect as a different position of the observer produces in the case of a comet on or near the equinoctial. We need scarcely add that the same law holds good for nearly every part of the heavens.

The reader who has carefully read the preceding remarks will be enabled to state exactly those conditions under which the tail of a comet will appear a straight line or a curve.

It must be borne in mind that the fact of a comet's tail *appearing* as a curve or arch to an observer on earth does not in the least prove that the tail of the comet is actually curved; it may be perfectly straight, and will yet appear as a curve from the reasons assigned.

There seems to have been some obscurity in the minds of former writers on astronomy on this subject of the curves of comets' tails, for we find the following statements relative to this curvature in the undermentioned works:—

"The tails of comets, too, are often somewhat curved, bending in general towards the region which the comet has left, as if moving somewhat more slowly, or as if resisted in their course."—*From Herschel's Outlines of Astronomy, Article* 557.

"The tails (of comets) appear to stream from that part of the nucleus which is furthest from the sun but seldom in the direction of a straight line joining the two bodies. They generally exhibit a sensible curvature."—*From Gallery of Nature, page* 112.

"P. Bienewitz, better known as Apian of Ingoldstadt, concluded from what he ascertained in the instance of the comet of 1531, that the tails are always opposite to the sun and in the direction of a line joining the centres of the sun and the comet; but more exact observation shows that as comets approach their perihelion the tails are gradually bent more or less towards the region they have left, as if there were a resisting medium. The tail of the comet of 1689 assumed the form of a Turkish scimitar; and that of the comet of 1744 was bent like a quarter of a circle."—*From Smyth's Celestial Cycle, vol.* 1, *page* 221.

"If no comet ever exhibited any other than this peculiar form of tail straight and directed from the sun, we might frame an hypothesis which could account for the facts; but in some instances there are many tails to the one nucleus,

and these not straight but curved like a scimitar."—*From Popular Astronomy. By O. M. Mitchell, LL.D.*

It is interesting to find how much attention has thus been given to the fact that comets' tails are usually curved ; but it seems that the geometrical laws which cause this curving have escaped observation. There is, however, one advantage derived from the problem, viz., that it appears it has prevented some hypotheses from being framed to account for what was *supposed* but *did not occur.*

It is most essential that before we invent a theory to account for what we see, we should be certain that we are not misled by mere appearances. We have demonstrated the cause of a comet's tail appearing in the heavens as a curve to one observer, whilst it appears at the same instant as a straight line to another observer at a different part of the earth ; and we have shown that this appearance is due to the geometry of the sphere.

It seems singular to find that so many writers whose popular reputation in connection with astronomy is great have by their remarks on this subject shown they were quite unacquainted with what we may term the practical geometry of the sphere, and with those elementary laws of apparent curves and apparent straight lines in the heavens which we have demonstrated in connection with the pole star and pointers and the curvature of comets' tails. This singular neglect of so important a study must not be passed over without some further remarks, which we bring forward for the purpose of eliciting truth, and with the object of checking if possible an evil in connection with science which seems to be spreading greatly and rapidly. This evil is, that there are a very large number of persons who pass for and call themselves astronomers, who consider that this science, based as it is on observation, and regulated as it is by geometrical laws, is to be considered solely as a subject to be treated by physical theories, and to be explained by a sort of dogmatic assertion or sentence, which if it contain some such words as "gravity," "attraction," or "repulsion," is supposed to be unanswerable.

When we find that even in the newspapers men write and state that the principal object of expeditions connected with the transit of Venus is to enable navigators to better find their latitude and longitude at sea, we at once know the writer to be ignorant of his subject. When, again, we find critics speaking of the rate of rotation of a planet being dependent on the laws of gravitation, we again know them to have but little knowledge of what these laws can

and cannot help us to discover. When, however, we find that a mere jumble of words, in which terms used in astronomy and in connection with physical science are mixed up together and put forward in a standard work intended for the guidance of the public as an explanation—when they have no real meaning as regards the facts they attempt to explain, we believe every true lover of science will agree with us that such proceedings should not only cease to be encouraged, but should be exposed and condemned.

We find also that the writers who thus attempt to explain by verbosity that which can only be solved by sound geometry almost invariably ignore facts. That the three angles of a plain triangle cannot be greater than 180° is to them of no consequence, and is to be overlooked if their not being greater interferes with some dogmatic theory. That these remarks are not uncalled for is, we believe, too well known to many really candid men of science. But that there should be no mistake on this point, we will now give one among many examples of the fact we wish to bring forward.

We have pointed out in this article that a comet's tail which coincided with the equinoctial would appear as a straight line or as a curve, according to the position of the observer on earth. We also demonstrated that a comet's tail which passed through the pole and was of considerable length would appear as a curve, and six hours afterwards as a straight line. The laws shown in these two cases hold good for the bending of comets' tails in all other parts of the heavens, and the *apparent* curvature is due to a geometrical law and not to any physical cause.

When then we find in a work of no less importance than the "Encyclopædia Britannica," under the article Astronomy, page 78, the following profound nonsense, put forward as an explanation of an effect due as we have demonstrated to a geometrical cause, it is evident how widely spread is this vicious system of explanation and how hollow and unsound are the scientific principles of those individuals who by such verbiage delude the ignorant and impose on the half scientific.

The following is the explanation given:—" The slight curvature which is generally observed (in a comet's tail) may be accounted for by combining the motion given to the vapours by the impulsion of the sun's rays with the motion of the comet in its orbit; for the detached vapours are driven by the impact of the luminous particles beyond the

sphere of the comet's attraction, and consequently cease to follow the direction of the nucleus."

This explanation needs no further comment; it is but a specimen of a class of so-called science which is in the present day very extensively followed. Just as a savage who has once seen an electric telegraph will hope to explain everything from the effervescence of champagne to the moving of a locomotive by attributing these effects to electricity, so his mental type in England endeavour to explain each astronomical effect by attributing it to the laws of gravitation, to "impulsion," "attraction," &c., causes which, as in the present case, have often nothing whatever to do with the question under consideration.

Other important problems connected with astronomy are dependent on these same geometrical laws, and will be treated of at another opportunity.

NOTICES OF BOOKS.

The History of Music. Vol. I. From the Earliest Records to the Fall of the Roman Empire. By W. Chappell, F.S.A. London: Simpkin and Marshall. 1874.

Of the arts of the Ancients—of sculpture, painting, music—sculpture is best known to us; the others are almost unknown. A few late frescoes, a few late hymns to gods and goddesses,—these are all that we can place side by side with the grand sculptures of Pheidias and Myron, with those wonderful architectural works the Propylæa and the Parthenon, the very ruins of which impress us with wonder, and show how perfectly the harmonies of the eye were understood twenty-two centuries ago. But the poetical and dramatic art of the most cultivated of ancient nations has also been presented to us; in that marvellous trio, Æschylus, Sophocles, Euripides, we behold a combination or concentration of the purest and most perfect art. We know how the gods were addressed, how their mysteries were celebrated, how Antigone lamented, and how Edipus passed suddenly and silently from the eyes of men when the dread voice at Colonos cried "Come hither, come." But we know not how the players played, and the choruses sang at divine celebrations, or when the crocus-coloured peplos was carried to the summit of the Acropolis amidst the rejoicing of the people, and the rhythmic dances of maidens with golden grasshoppers in their hair.

We may, however, congratulate ourselves that we have in the work before us all that is known of Greek music, and much that has not been known before,—a Greek hymn to Nemesis harmonised by Macfarren, an explanation of Greek notation, Greek scales, and Greek singing; of *Harmonici* and *Kanonici*.

Histories of ancient music are not common, although, so long ago as 1581, Vincenzo Galilei published his "Dialogo della Musica Antica." The works best known to us in this country are those of Sir John Hawkins (1776) and Dr. Burney (1776—1789). The work of the former was cumbrous and unreadable, and met with but little favour, while Dr. Burney unfortunately relied to a great extent on Boethius for his knowledge of ancient music. But the treatise of Boethius was a broken reed, for its author utterly misunderstood Greek music, and concluded that the words *nete* and *hypate* (signifying respectively the shortest and the longest string of the seven-stringed lyre) referred to the top and bottom of the scale; thus he turned the Greek scale upside down. Yet the work of Boethius, "De Institutione Musica," was the text-book during

the earlier Middle Ages, and till the time of Guido d'Arezzo, in the eleventh century.

The Greeks acquired their music from the Egyptians; *Renaissant* Europe acquired its music from Greek sources. "No Roman of antiquity," says Mr. Chappell, "is known to have made, or even to have attempted, any improvement in the science of music. The Romans received the diatonic scale, of tones and semitones, from the Greeks, at a time when it existed only in its primitive and imperfect form. Nevertheless they were content to retain it so, and did not follow the Greeks in any subsequent improvement. It is for that reason Greek music cannot be effectually learnt from Roman writers." In the fourth century the practice of singing alternate verses of psalms by a choir was introduced from Antioch, and this was called *antiphonal.* The introduction of Greek words in ecclesiastical music was another cause of confusion in the study of Greek music; for the meaning of *anti,* as applied to Greek music, is in the sense of *with* or *accompanying,* not *against,* and Greek antiphons were harmonious and concordant sounds, an octave apart. Again, according to Mr. Chappell, the Greek *Harmonia* means a "system of music," or simply "music," and has nothing to do with the modern word "harmony," either in its English, French, German, or Spanish sense. Also, the Greek *Melodia* does not mean melody according to our sense of the word. "Greek *Melos* had not necessarily any tune in it. It applied to the rising and falling sounds of the voice when linked together in speech, or in rhythm, as well as in music; so that recitation, without any musical intervals in it, would still be *melodia.* Thirdly, *Harmonike* does not mean *harmonic* or *harmonics,* but is a synonyme for *harmonia.* Again, *Sumphonia* does not mean 'symphony.' The last expresses our 'harmony,' viz., concords of notes of different pitch." No wonder that Greek music has puzzled so many modern writers; no wonder Dr. Burney calls it "a dark and difficult subject, which has foiled the most learned men of the two or three last centuries."

Our present musical scale is founded upon that of the Greeks; it is simply a re-adjustment of the Pythagorean scale, made in the second century A. D., by Claudius Ptolemy, the mathematician. The Greeks trace the origin of their music to Hermes, who invented the lyre. The old Homeric lyre was an instrument of four strings, and several accounts are given of its invention by Apollodorus, Diodorus Siculus, and others. According to a hymn to Hermes, of considerable antiquity, the god "found a mountain tortoise grazing near his grotto on Mount Kyllene. He disembowelled it, took its shell, and out of the back of the shell he formed the lyre. He cut two stalks of reed of equal length, and, boring the shell, he employed them as arms or sides to the lyre. He stretched the skin of an ox over the shell. It was perhaps the inner skin, to cover the open part, and thus to

give it a sort of leather or parchment front. Then he tied cross-bars of reed to the arms, and attached seven strings of sheep-gut to the cross-bars. After that he tried the strings with a plectrum." According to another account, the lyre was invented by the Egyptian god Thoth, who, while walking along the shores of the Nile, happened to strike his foot against the shell of a dead tortoise, containing nothing within its shell but the dried cartilages of the animal. He was so pleased with the sounds produced that he constructed a musical instrument from the same shell, and put strings to it of dried animal sinews. It was with the lyre of Hermes that Orpheus charmed Creation, and even tamed the furies of Hades.

The earliest representations of musical instruments and of concerts are to be found among the frescoes and papyri-paintings of ancient Egypt. No less than thirteen different musical instruments have been noticed in various papyri:—Harp, lyre, lute, flute, double pipes, sistrum, &c. A conductor is often seen beating time with his hands; sometimes one conductor for the instruments, another for the chorus. Some of these representations go back as far as the time of Moses. "We may trace the prototype of every Greek instrument in Egypt. No kind of advance upon the music of that ancient country seems to have been made till the three Alexandrian mathematicians, Eratosthenes, Didymus, and Claudius Ptolemy, appeared successively upon the scene, and improved the scale. Eratosthenes, the first of them, was born about 276 B.C. He was director of the Alexandrian Library."

Pythagoras is believed to have imported the octave system from Egypt into Greece. We all know the old story of his having discovered it: when passing a blacksmith's shop he heard the consonances of the fourth, fifth, and octave, and weighed the hammers. The other story of the strings is less trite, but equally untrue. It is said that Pythagoras took strings of equal thickness and length, fixed them at one end, passed them over a bridge, and weighted them at the other end with weights of 6, 8, 9, and 12 pounds. But Galileo pointed out that in order to get the octave, the fourth, and the fifth, from such an arrangement, it would be necessary that the weights should be the squares of 6, 8, 9, 12. The doctrine of the Harmony of the Spheres was based upon the octave system of music. The earliest extant notice of this system is found in some fragments attributed to Philolaos, who was the first to make known many of the doctrines of Pythagoras.

Greek singing, according to Mr. Chappell, must often have severely strained the voice. In the "manly and severe" Dorian scale the key-note was the tenor *d* in the space below the treble clef. Aristotle says that few persons could sing the νομοι 'ορφιοι, addressed to Apollo, on account of their high notes. "Apollo seems to have been addressed as if he had been troubled with

deafness, or was supposed to be a long way off; and perhaps that was the general style of Greek antiquity. It recalls Elijah's mockery of the priests of Baal—telling them to 'cry aloud: peradventure he sleepeth, and must be awakened.'" But for ordinary purposes the Greek compass was much the same as that of the present day. The various Greek octaves were called Lydian, Phrygian, Dorian, &c., by Euclid, Gaudentius, and other writers.

Any time during the last two centuries the question whether the Greeks practised simultaneous consonances mixed with discords (what we call *harmony*) has been discussed. It was discovered that Ἁρμονία does not mean simultaneous concordant sounds: συμφωνια is the word for simultaneous consonance, but this was generally not allowed, "If the enquiry had been pursued in the only proper way, by searching for and comparing Greek definitions of *harmonia*, its meaning would inevitably have been traced to the theory and practice of music, and identical with the later word Harmonike. *Harmonia* includes poetry united with music, but not poetry alone, and so it has a more restricted sense than Mousike. Again, the chanting of poetry, though unregulated by musical intervals, is *melodia*, and the metre of poetry brings it within the denomination of Mousike; but it is not *harmonia*. So that the primary translation of the word *harmonia* is our 'music.'" Mr. Chappell quotes various ancient authors to prove that harmony was well known to and employed by the Greeks. He quotes, among other authors, Seneca and Cicero: the former says—"And now to music: you teach how voices high and low make harmony together, how concord may arise from strings of varying sounds." (Is not this rather a free translation Mr. Chappell? "*Doces me quomodo inter se acutæ et graves voces consonent, quomodo nervorum disparum reddentium sonum fiat concordia.*") "Teach rather how my mind can be in concord with itself, and my thoughts be free from discord." Cicero, in the second book of the "Republic," says—"For as in strings or pipes, or in vocal music, a certain consonance is to be maintained out of different sounds, which, if changed or made discrepant, educated ears cannot endure; and as this consonance, arising from the control of dissimilar voices, is yet proved to be concordant and agreeing, —so, out of the highest, the lowest, the middle, and the intermediate orders of men, as in sounds, the State becomes of accord through the controlled relation, and by the agreement of dissimilar ranks; and that which, in music, is by musicians called *harmony*, the same is concord in a State." Mr. Chappell considers that Cicero's mere definition of the word *concentus*, and the assertion of Aristotle that "all concordant sounds are more agreeable than single notes, and of concords the octave is the most agreeable," are alone sufficient to prove that the Greeks were acquainted with harmony. "But," he adds very happily,

"floating upon the surface of music has been for ages more popular than diving."

We come now to the eighth chapter of the "History," which is important, inasmuch as it contains three Greek hymns, written out in accordance with our modern notation, and supposed to be the only trustworthy remains of Greek music. They were first published by Vincenzo Galilei, the father of the astronomer, in 1581, and were copied from Greek MS. in the library of Cardinal St. Angelo. The date of the hymns is uncertain, but it is not considered probable that they are older than from the second to the fourth century, A. D. The first is a hymn to Calliope, with an accompaniment in the Hypo-Lydian mode by George Macfarren; the second a hymn to Apollo, both grave and solemn, and something like some of the graver hymns of the Catholic Church; the third, a hymn to Nemesis, is by far the most striking of the three, and contains a very definite and distinctive theme.

A curious example of the delicacy of the human ear is to be found in the fact that the so-called "Comma of Didymus," the interval between a major and a minor tone, or between the 80th and 81st part of a vibrating string, sufficed to produce the great change between the scale of Pythagoras and the scale which we now use. The harmonic scale was developed during the last century; it was discovered in 1673, by William Noble, of Merton College, and Thomas Pigot, of Wadham. It is the scale of natural sounds arising from the successive aliquot divisions of a string, and, according to Mr. Chappell, it forms the basis of the science of music.

The last four chapters of Mr. Chappell's book are devoted to an account of the musical instruments of the ancients:—Various kinds of pipes, and the way in which they were sounded, and the materials of which they were made—lotus, laurel, palm-wood, pine-wood, box-wood, beech-wood, elder-wood, ivory, reeds of various kinds, leg-bones of animals and of large birds (such as the eagle, vulture, and kite). Then there were pipes receiving their name from the country in which they were invented—as Alexandrian, Tuscan, Theban, Scythian, Phœnician, Lybian, Arabian, Phrygian. The Theban pipes were made of the thigh-bones of a fawn, and were covered with metal. "The length of Arabian pipes was proverbial, and a man of whose tongue there seemed to be no end was called an Arabian piper." The "sweet monaulos" was for weddings, and the Phrygian pipes were "for wailing and lamenting." After wind instruments come instruments of percussion, such as the drum, dulcimer, timbrel, sistrum, cymbals, and krotala. According to Mr. Chappell the sistrum was simply shaken, so as to produce a jingling sound; Plutarch has given a long account of the instrument. The Assyrians appear to have had a sistrum of a different form, which was struck by a rod of metal serving as a plectrum.

The Greeks appear to have possessed three kinds of cymbals. As for stringed instruments, their number was considerable: there were many kinds of lyre, harps, psalteries, four-stringed trigons, and guitar-like instruments. The largest kind of lyre had many strings, and was placed upon a stand; the others were carried in the hand. Athenæus quotes a singular story told by Artemon, to the effect "that Pythagoras once strung the three sides of a Delphian tripod, such as was used to support an ornamental vase, and that he tuned one side to the Dorian scale, another to the Phrygian, and the third to the Lydian scale or mode. So far all was possible, but it is improbable that Pythagoras should have attempted it, because there could be no tone from such a tripod, for it had no sounding-board. The minuteness of the remaining part of the story proves the whole to be a myth. Artemon adds that Pythagoras contrived a pedal to turn this tripod, and that he twisted it about with such rapidity while he was playing that any one might have fancied he was hearing three players upon three different instruments."

Mr. Chappell considers the Greeks to have been very uninventive in regard to musical instruments, which all seem to be Asiatic or Egyptian. The lyre is found in Egyptian paintings before the Greeks existed as a nation. "We can find no new principle for stringed instruments discovered by a Greek, nor anything new in pipes. All was ready-made for them, together with their system of music. The Greeks were even inapt pupils; for, although they had many strings ever before their eyes, they did but reduce the number, after a time, to bring the instruments down to their own level. They practised a certain amount of harmony, but not so much as earlier nations." Our author even goes so far as to compare Greek music of an early date with modern Japanese music, of which a curious account is given on p. 304. We are glad to notice representations (pp. 314—315) of the beautiful harps from the tomb of Rameses the Third.

The thirteenth and final chapter of the work is devoted to organs, and Mr. Chappell has carefully investigated the various forms of hydraulic and other organs described by Hero in his Πνευματικα. He has even had a model of one of them constructed, and points out its advantages. The invention of the hydraulic organ is attributed to Ctesibius, who appears to have lived about 284 B.C. The precise meaning of the word οργανον has sometimes led to confusion. It was often used to mean any *instrument;* it might be a surgical instrument, or a musical instrument, or an organ of sense,—as the instrument of reasoning,—and so on. The first description of the hydraulic organ is given by Hero of Alexandria, who was a pupil of Ctesibius. A second full account of it is given by Vitruvius, in his Treatise on Architecture, written between B.C. 20 and 11. Many of the early editions of the "Pneumatics" appeared with drawings,

but none of these are said to be older than the fourteenth or fifteenth century. A capital drawing of the organ is given on p. 340. It had keys, pipes, valves, a wind-chest, and a pump for condensing air into the wind-chest, and even stops. The Romans, in the time of Nero, appear to have had organ-contests, and medals were given as prizes, one of which is in the British Museum.

Mr. Chappell's work is altogether interesting, and contains a good deal of new matter. A few of the chapters appear to be unnecessarily complex, and perhaps can be somewhat simplified in a second edition. A few errors exist, which may also then be altered: perhaps the most important of these is to be found in the foot-note to p. 187-188, in which Lissajous' Figures are wrongly described as produced by sand on vibrating plates. The book is well printed and illustrated, and we shall look forward with interest to the appearance of the remaining volumes.

The Harveian Oration for 1874. By CHARLES WEST, M.D., F.R.C.P. London: Longmans, Green, and Co. 1874.

THE practice of giving the annual Harveian Oration in Latin has been discontinued for several years; and in this country it is not easy to hear a Latin address, save on certain occasions in our older Universities. In Leyden, and in some of the German and Italian Universities, it is, however, a far commoner practice. The present oration could well be converted into somewhat florid and verbose Latin: for example, think of such resounding periods as the following:—"When your commands, Sir, were first laid upon me to undertake this most honourable, most arduous office, I studied, as a preparation for its accomplishment, all the Harveian orations that I could meet with." Or, again—"Such was the man, such were his pursuits; loving knowledge for its own sake; loving it, too, not in pride of intellect," &c. The oration relates to the Life and Times of Harvey. It tells us that he was born in 1578, that he left school and entered at Caius College at the age of 15, and took his B.A. in 1597. On leaving Cambridge he went to Padua, then a most celebrated University and Medical School. At one time it possessed no less than 18,000 students, and it numbered among its Professors some of the most eminent men in Europe. Harvey passed nearly five years at Padua, and took his degree in 1602; on returning to England, he became M.D. of Cambridge in 1603, F.R.C.P. in 1607, married and commenced practice in London, and in 1609 became Physician to St. Bartholomew's Hospital, and then he worked hard at anatomical discoveries. His greatest discovery is described by Dr. West as follows:—"*First.* After corroborating the statements of those who had

denied either that blood transudes through the walls of the ventricles, or that the pulmonary veins bring back to the left side of the heart air commingled with the blood, he asserts that the left ventricle has no other function than that of impelling the blood brought to it through the arteries, which themselves contain blood and nothing else, not air, nor vital spirit, but blood purified by its passage through the lungs, and so made apt for the nourishment of the whole body. And *second*. That while the arteries thus distribute everywhere the fresh pure blood, the veins with which they communicate bring back that same blood, no longer pure, to the right side of the heart, whence it is once more transmitted to the lungs, thence carried again revivified to the left ventricle, and then once more distributed throughout the body, its changes not being those of an ebbing and a flowing tide, but the ceaseless current of an onward rushing river." The doctrine of the circulation of the blood was taught as early as 1615 by Harvey, but it was not till the year 1628 that he published his "Exercitatio de Motu Cordis." The conclusion of the oration is devoted to a somewhat graceful allusion to benefactors of the College:—"Benefactors—those who have done us good, or have shown us kindness; in vulgar sort, those to whom we owe gifts of money, grants of land, something or other that can be bought or sold in open market. According to this reading, few indeed have been our benefactors." All the money properties of the College do not exceed £600 a year, and the plate not more than £20. The College, says Dr. West, will bear no comparison with the magnificent Halls of the City Companies; and then later on he says—"Our great benefactors are they who have left us the inheritance of their example." Such as Sydenham, and Meade, Jenner, and Bright, and pre-eminently Harvey.

Divine Revelation, or Pseudo-Science? An Essay. By R. G. Suckling Brown, B.D. London: Longmans, Green, and Co. 1874.

This is one of the many anti-Darwinian books, which the author calls "our form of resisting the hypothesis of evolution, and kindred pseudo-sciences." We fear we must pronounce Mr. Brown's book to be illogical and unscientific, and of little value even to the opponents of the Darwinian theory. We give the concluding lines, and so leave the book to the judgment of the reader:—"The admirable adaptation of the bird's wing; the contrivance of the human fore-arm, hand, and foot; the poise of suns and planets in infinite space: their accelerated and diminished velocities in the ratio of their distance from their centres of attraction or gravitation; the apparatus of the infusorial *Rotifer*, the tyrant over the inhabitants of a drop of water; the delivery of its contemplated victim by its provided apparatus for

withstanding the torrent, stirred by its tyrant's paddles; all these great, minute, and wondrous; all these most curious and admirable works of an Almighty Designer and Creator, who governs by compensation, and tempers compensation by instituting a maximum of enjoyment through a minimum of suffering; who maintains His creations by the law of self-preservation: all these, and multitudes of other benignant fore-thoughts, these busy physiologists overlook, neither thinking, nor asking, nor, they give us reason to imagine, caring to know 'Whose works are these?' Nay, more! while they are loquacious of acids and salines, they forget to think out or to ask 'Whose works are these?' Who made acids and salines? And while they trace '*the human animal*,' for such they account man to be, and such they would make him, to the askidion or the monad, they consider not, and forget to ask, 'Who made the monad and the askidion?'"

The Correlation of Physical Forces. Sixth Edition. With other Contributions to Science. By the Hon. Sir W. R. Grove, M.A., F.R.S., one of the Judges of the Court of Common Pleas. London: Longmans, Green, and Co. 1874.

We are very glad to welcome a sixth and revised edition of Sir W. Grove's celebrated Essay. By "revised" we mean simply that those discoveries in Science which bear upon the subject of the relationship of the physical forces made since the first appearance of the Essay, in 1843, have been added, and the whole has been thus rendered more complete. It may be said that the Essay has done its work, that scientific men fully realise the correlation of the physical forces, which has been so wonderfully exemplified during the last thirty years by the science of thermo-dynamics, but the Essay still remains a model of accurate reasoning, and of an elegant scholarly style, and will always be studied with advantage by the student of Science. We need not give a very detailed account of its object; it is too well known to require that. But, in brief, the design is to show that if we take the so-called physical forces,—motion, light, heat, electricity, magnetism, and chemical affinity,—each one is capable of producing the remaining five; and, further, from first to last the author combats the idea of such hypothetical entities as ethers, subtle fluids, &c., which have been devised to account for phenomena otherwise not easily explained. He endeavours to show that each and all of the physical forces is an affection of matter, not matter itself,—that, in fact, it is a mode of motion; and this idea, now fully admitted in the case of heat and light, will, no doubt, in our generation be extended to electricity and magnetism.

Without giving any very connected chain of reasoning in a

book which has long been before the public, we will indicate here those passages which are striking from their originality or precision, or from some other cause. As to causation, our author remarks that the view entertained of it is usually that of Hume, who refers it to "invariable antecedence," a cause being that which invariably precedes, and in effect that which invariably succeeds; and he shows that this definition will not bear strict scrutiny. As to the aims of physical science, he remarks—"Instead of regarding the proper object of physical science as a search after essential causes, I believe it ought to be, and must be, a search after facts and relations; that although the word Cause may be used in a secondary and concrete sense, as meaning antecedent forces, yet in an abstract sense it is totally inapplicable."

Form is defined as "that active principle inseparable from matter which is supposed to induce its various changes,"—not, to our mind, a satisfactory definition, although we know not where to look for better. Referring to "force in abeyance," or tension, or what we should now call "potential energy," Sir W. Grove gives us the following comprehensive and perfectly logical remarks:—"But it may be objected, if tension or static force be thus motion in abeyance, there is at all times a large amount of dynamical action subtracted from the universe. Every stone raised and left upon a hill, every spring that is bent and has required force to upraise and bend it, has for a time, and possibly for ever, withdrawn this force, and annihilated it. Not so; when we raise a weight and leave it at the point to which it has been elevated, we have changed the centre of gravity of the earth, and consequently the earth's position with reference to the sun, planets, and stars; the effort we have made pervades and shakes the universe; nor can we present to the mind any exercise of force which is not thus permanent in its dynamical effects. If, instead of one weight being raised, we raise two weights, each placed at points of the earth diametrically opposite to each other, it would be said, here we have compensation, a balance, no change in the centre of gravity of the earth; but we have increased the mean diameter of the earth, and a perturbation of our planet, and of all other celestial bodies, necessarily ensues."

The definition of heat as "a communicable molecular repulsive force" would scarcely be very generally accepted, we think. A repulsive force cannot readily be defined, or in the abstract be conceived; but if we regard heat as vibratory motion, we can at once realise how expansions are produced by the addition of such motion to congeries of molecules, which vibratory motion will of necessity drive them further apart.

Of electricity, Sir W. Grove says—"I think I shall not be unsupported by many who have attentively studied electrical phenomena, in viewing them as resulting not from the action of a fluid or fluids, but as a molecular polarisation of ordinary

matter, or as matter acting by attraction and repulsion in a definite direction."

The remarks to be found on pp. 106 and 107, relating to the finite nature of our intellects, are much to be commended to the notice of scientific men, at a time when perhaps as much as in that of the Greek Positivists, or more modern Encyclopedists, intellectual pride is the bane of too many otherwise great minds:—"Men are too apt," remarks our author, "because they are men, because their existence is the one thing of all importance to themselves, to frame schemes of the universe as though it was formed for man alone: painted by an artist of the Sun, a man might not represent so prominent an object of creation as he does when represented by his own pencil."

Sir W. Grove often leads us from a purely physical to the verge of metaphysical discussion. In regard to the boundaries of the universe, he says—"We cannot conceive a physical boundary, for then immediately comes the question, What bounds the boundary? and to suppose the stellar universe to be bounded by infinite space, or by infinite chaos, that is to say, to suppose a spot—for it would then become so—of matter in definite forms, with definite forces, and probably teeming with definite organic beings, plunged in a universe of nothing, is to my mind, at least, far more unphilosophical than to suppose a boundless universe of matter existing in forms and actions more or less analogous to those which, as far as our examination goes, pervades space."

The last of the physical forces which is considered is chemical affinity, of which it is truly observed that it is that mode of force of which the human mind has hitherto formed the least definite idea. We think that the weakest part of this Essay is that in which the other physical forces are said to produce chemical affinity: thus we read on p. 26—"In the decompositions and compositions which the terminal points proceeding from the conductors of an electrical machine develope when immersed in different chemical media, we get the production of chemical affinity by electricity, of which motion is the initial source." Again, light may effect chemical combinations and decompositions, but it does not *produce* chemical affinity as a force; it simply determines a chemical change, or determines a manifestation of chemical force, which had all along been existent in the bodies which undergo the change. We find, among the concluding remarks, a statement of instances in which some one force being disturbed, all the others appear:—"Thus, when a substance, such as sulphuret of antimony, is electrified, at the instant of electrisation it becomes *magnetic* in directing at right angles to the lines of electric force; at the same time it becomes *heated* to an extent greater or less, according to the intensity of the electric force. If this intensity be exalted to a certain point the sulphuret becomes luminous, or *light* is produced: it expands

consequently, *motion* is produced, and it is decomposed, therefore *chemical action* is produced." This is a kind of *tutti*, after each one has been singing his solo.

Few words are used more loosely and indefinitely than the word *Nature;* it is sometimes called "the principle which produces all things;" hence, poetically, the "Universal Mother," "Madre Natura," &c. We remember to have read, in one of the older numbers of the "Philosophical Transactions,"—"By Nature I understand the works of God manifested in Creation;" then we talk about the "laws of Nature," the "works of Nature," &c. Sir W. Grove says (p. 152)—"The word 'Nature' is still more personified; instead of being used to denote, what alone it can denote,—namely, things as we see, hear, or feel them, and their relations ascertained by comparison and abstraction,—Nature is treated as a sort of Superintending Angel, who enjoins this, permits that, and forbids the other." Finally, as to matter and force, he says—"The evidence we acquire of the continued existence of matter is by the continued exertion of the force it exercises, as, when we weigh it, our evidence of force is the matter it acts upon. Thus matter and force are correlates in the strictest sense of the word; the conception of the existence of the one involves the conception of the existence of the other: the quantity of matter, again, and the degree of force, involve conceptions of space and time."

The Address "On Continuity," which has already reached a third edition, was delivered by Sir W. Grove, as President of the British Association, at Nottingham, in 1866. It is, to our mind, quite a model of what a President's Address should be. Not a vehicle for the exclusive advocacy of Darwinism and free thought, but a *resumé* of the whole aspect of Science at that particular time, and an allusion to all the more dominant phases of thought which then prevail, and to the more prominent discoveries and generalisations. These are interspersed with his own far-seeing and luciferous ideas:—"As Phlogiston," he says, "and similar creations of the mind have passed away, so with hypothetic fluids, imponderable matters, specific ethers, and other inventions of entities, made to vary according to the requirements of the theorist, I believe the day is approaching when these will be dispensed with, and when the two fundamental conceptions of matter and motion will be found sufficient to explain physical phenomena."

The remainder of the work (that is to say, half of it) contains Sir W. Grove's researches in experimental science. Commencing with the nitric acid battery, now so well known and so much used, and which was devised in 1839, we find, in succession, the gas battery, the decomposition of water by heat, the striæ in the electrical discharge, and the effects of heat on fluids. There are many other papers, all containing a good deal of suggestive matter, and hints as to new researches.

We know of no book, save the admirable "Experimental Researches" and "Chemical Manipulation" of Faraday, which we could more recommend, both to the general reader of Science and to the young student, than this: clear and elegant in style, powerful in grasp, close and precise in reasoning, eminently suggestive, and very comprehensive, the book will continue to be a standard scientific work, and will always find a welcome place among the archives of the History of Scientific Ideas.

The Universe and the Coming Transits. Presenting Researches into, and New Views respecting, the Constitution of the Heavens; together with an Investigation of the Conditions of the Coming Transits of Venus. Recently confirmed by a Unanimous Vote of the Chief Astronomers of Great Britain. By RICHARD A. PROCTOR, B.A. (Camb.). London: Longmans, Green, and Co. 1874.

MR. PROCTOR is certainly the most prolific scientific writer of the age; he is a very Lope de Vega among scientists: book after book appears from his pen; his thoughts fly out as the sparks fly upwards. Sun, moon, and stars have alike received detail treatment at his hands; and here we have a new theory of the universe, and a collection of all the matter relating to the transit of Venus which he has published in the "Journal of the Royal Astronomical Society," and elsewhere. The book, in common with all his works, is very profusely illustrated; maps, charts, plans, woodcuts, are scattered throughout the volume,—for Mr. Proctor is a good draughtsman as well as a learned astronomer.

The first Essay, "On Star-Streams," treats of the Milky Way, which is now believed to consist of myriads of suns, around which no doubt there are attendant planets, which latter may contain countless numbers of living creatures. A very interesting account is given of the changes in the appearance and position of the Milky Way at the same hour, in different seasons of the year. By a simple illustration of the appearance that the lights of a large town would present to anyone looking from a height, we are shown that we must not necessarily suppose that the brilliancy of a star is positive evidence of *proximity*, but rather must bear in mind that we are dealing with a sphere *full of stars*, distributed through space, some at much greater distances from us than others, and *not* with a spherical surface *covered* with stars, as represented by a globe or map. For aught we know the whole stellar region may be occupied, more or less richly, by a variety of forms of matter other than stars or suns, and nothing but the most patient and systematic analysis can ever lead to the solution of the great problem of star-depths.

The article entitled "A New Theory of the Universe" is at

once the longest and the most interesting contained in the volume: it was originally brought out in "The Student" for February, March, and April, 1869; therefore many discoveries and some extensions of the subject have been made since, but this in no way interferes with the correctness of the paper. Mr. Proctor begins by a comparison of what was known of the solar system at the end of the last century with what is known at the present time: first among the remarkable discoveries made within the last sixty years must be noticed the great increase in the number of primary attendants upon the sun; the 98th asteroid was just discovered at the time this Essay was written, and yet it was but on the opening day of the present century that the first of these bodies was brought to light. The mind gets bewildered in imagining all these bodies, primaries of the planetary system, revolving in paths closely interwoven, the more so when—given these constant additions to the numbers known—we may fairly assume that for each discovered asteroid there are to be reckoned tens, perhaps hundreds, which will never be found out. Then the myriads of dependent comets that are found to exist within the solar system, revolving round the great centre in the most eccentric orbits, must still remain among the mysteries of Science. At the present time, when the minds of even the most unscientific among us have been more or less interested by the appearance of a comet, this branch of the subject presents a special attraction, and it is most ably treated. A still more remarkable feature of modern astronomical discovery remains to be noticed; that is, that meteors, shooting stars, and aërolites, which have always been regarded as meteorological phenomena, must now be placed among the attendants of the sun. It is beyond doubt that the earth encounters fifty-six systems at least of these small bodies, and "What is the likelihood," asks Mr, Proctor, "that if there were only a few hundreds of such systems the earth would encounter so many as fifty-six?" The extreme probability, nay certainty, is that such systems may be reckoned not by hundreds and thousands, therefore, but by millions of millions. A close connection, if not identity, can be traced between comets and shooting star systems, as it is discovered that at least two of the meteoric systems coincide with the orbits of known comets. The Sidereal System next engages the attention, and the last twenty pages of this Essay are devoted to the consideration of what our astronomer terms "those mysteries of mysteries," the nebulæ. The entire paper deserves careful and attentive study.

The next article, bearing the title "What Fills the Star Depths?" is almost entirely devoted to mathematical problems proving that the arrangements of stars in different regions, &c., is not the result of accident, but rather that some real laws of aggregation exist among the stars. And then we come to an Essay of great interest, headed "Star-Drift," and treating—as

the name implies—of stellar motion, particularly of the motion of the "sun, with his whole *cortege* of planets and cometary systems sweeping swiftly through space." This star-drift, however, must not be confounded with the phenomenon of drift as applied to Ursa Major and other constellations, and treated of in another part of the same paper. The star-drift due to the sun's motion in space may be called *general*, and has altogether a different significance to the other star-drift, which is only *local*. The one takes place in almost exactly the reverse direction from the other, and is perfectly distinct. In this chapter Mr. Proctor more than redeems a little self-assertion observable elsewhere, by his cordial expressions of admiration of the labours of his great countrymen, the Herschels.

The following Essays, "Are there any Fixed Stars?" "News from the Stars," "On two Rich Nebular Regions," &c., are reprinted from various sources, and discuss the most recent problems in Astronomy. The Essay "On the Construction of the Heavens," containing Jean Paul's wonderful dream of the universe, appeared in this Journal in July, 1872.

The second part of the volume—about one-third—treats exclusively of the coming transits of Venus, and is more technical, and consequently less popularly interesting, than the former portion of the work. The first transit will take place on December 8th of the present year; the second on December 6th, 1882; and both have such important bearing upon the problem of the sun's distance that men of science are anxiously looking forward to the times of their occurrence. After the present century no like phenomenon will occur until the year 2004; and in one respect the transit now at hand will present even better opportunity for making important calculations than that of 1882, so that for a number of years astronomers will be without the means of remedying any omissions now made: it is this consideration that induces Mr. Proctor to appeal most earnestly to his country to send out expeditions suitably furnished for making the most effective observations for the determination of the sun's distance: he feels that the scientific honour of his country is at stake, and most clearly and ably shows the manner in which it may be saved. There seem to be difficulties in the way of carrying out many of these observations, but it is greatly to be desired that the subject will meet with the recognition it deserves: any omission in making the proper arrangements for this important work might be obviated by due attention to the details so clearly set forth by Mr. Proctor; and we cannot close his book without a feeling of admiration for the manner in which he has thought well to bring his researches thus before the public, and for his perseverance and industry.

Handbook of Natural Philosophy. By DIONYSIUS LARDNER, D.C.L. Hydrostatics and Pneumatics. Edited and the greater part re-written by BENJAMIN LOEWY, F.R.A.S. London: Lockwood and Co.

IN sciences like Hydrostatics and Pneumatics, on whose facts there is little, if any, variety of opinion, and which have been undisturbed by any recent revolution in theory, or even in nomenclature, the compilation of handbooks is comparatively simple, and there is accordingly very little scope for criticism. The work before us is well arranged and clearly written, and may be recommended to all who require a knowledge of the subjects treated of without having the time or the inclination to enter upon mathematical refinements. Purists in methodology might, perhaps, raise the question whether the consideration of the diffusion of liquids, of the division of soluble bodies into crystalloids and colloids, and of the phenomena of osmose, can be legitimately introduced into a treatise on hydrostatics, involving, as they do, chemical principles.

Plattner's Manual of Qualitative and Quantitative Analysis with the Blowpipe. Revised and enlarged by Prof. TH. RICHTER. Translated by H. B. CORNWALL, assisted by J. H. CASWELL. Second Edition, revised. New York: D. Van Nostrand.

PROF. PLATTNER is generally recognised as *facile princeps* among authorities on blowpipe analysis, and his great work has passed beyond the domain of criticism. Of all the editions which have appeared in the English language, the present must claim the palm. It is founded upon the most recent German edition, as enlarged and revised in accord with the latest discoveries by Prof. Richter, of the Freyberg Mining School, the worthy pupil of Plattner. The translation has been carefully and judiciously executed. We are happy to say that there has been no attempt at abridgment, a thing impracticable without sacrificing that thoroughness which was a characteristic of Prof. Plattner, and on which the main value of his work depends.

We hope that this edition may be duly appreciated, and consider that it cannot fail to lead to a wider knowledge of the utility and the resources of the blowpipe.

Theory of Arches. By Prof. W. ALLAN. New York: D. Van Nostrand.

THIS work forms one of the "Science Series" of the eminent publisher by whom it is issued. It is an expansion of the views

of the late Prof. Rankine upon this subject. The writer develops the theory of arches by beginning with a consideration of forces acting upon a suspended chain or cord. We have no doubt that it will prove of great value to architects and engineers.

Geology. By T. G. Bonney, M.A., F.G.S. London: Society for Promoting Christian Knowledge.

The author of this little treatise has, as he tells us in the Preface, "attempted to set down briefly the principal facts of Geology, and the conclusions which have been drawn from them; to indicate the nature of the earth's crust, the processes which have acted and are still acting upon it, and the probable history of that little portion upon which we live." The attempt has been by no means unsuccessful, and the result is a work well calculated, we believe, to lead to further enquiry. With the following remark we most heartily concur:—"The great aim of the Natural Sciences is to teach students to observe and think for themselves: when this result is not produced they are mere cram, and do more harm than good."

The wondrous tale written on the rocks is told here in the simplest possible words, and technical language, if not entirely avoided, is kept subordinate. The author, we should say, is not one of those men of science who would write a book merely for the sake of displaying a "fire-new" nomenclature. The very appearance of this work, and the frank admission not only of the incalculable age of the world we inhabit, but of the antiquity of the human race, is an instructive sign of the times. Had such a work been issued, under such auspices, half a century ago, certain of our contemporaries would—if such a thing be possible —have out-scolded and out-shrieked Louis Veuillot and Joseph de Maistre.

We wish this book a wide circulation, and feel convinced that it will have a good effect on the public mind.

Fourth Annual Report of the Board of Commissioners of Public Charities of the State of Pennsylvania. Harrisburg: B. Singerly.

This report contains much interesting information on the management of reformatories, criminal schools, and lunatic asylums in Pennsylvania. The treatment of pauper lunatics in some of these establishments appears to be shocking (cxlvii.). It seems from the "suggestions," p. 105, that in America persons who have been so unfortunate as to witness the commission of a crime may be committed to prison—if unable to find security for their

appearance—in order to secure their evidence at an approaching or postponed trial. Apart from the obvious injustice of such a system, we think it must frustrate the enforcement of the law, by furnishing those who are in any way aware of the perpetration of a crime with a reason for keeping silence even more potent than the insolence and the brow-beating which witnesses, except persons of rank and station, have to suffer from counsel in England.

To statisticians, philanthropists, and persons interested in the management of asylums, hospitals, gaols, and poor-houses, this volume will prove invaluable.

Mineralogy. By Frank Rutley, F.G.S. London: T. Murby.

This work belongs to "Murby's Science and Art Department Series of Text-Books," edited by B. J. Skertchly, F.G.S. What share, if any, the last-named gentleman has had in the production of the treatise, or to what extent he is responsible for its contents, it does not appear. The work, as we are told in the Preface, is destined to meet "the requirements both of elementary and advanced students for the Science and Art Department Examinations." It may, perhaps, be looked upon as an old-fashioned prejudice, but we like people to study any subject whatever in order to know, and not in order to pass examinations. We dislike cram, crammers, and crammees. We have no sympathy for the man into whom a certain dose of science has been forced at high pressure, like carbonic acid into a bottle of soda-water. When the pressure is removed, the science in the one case and the gas in the other escape with noise and effervescence, leaving a residue stale, flat, and unprofitable. But whilst thus taking exception to the declared object of the work, we must speak favourably of its contents. It might prove a difficult task to present a greater amount of useful matter in so limited a compass. The distinctive features to which the author lays claim are the general arrangement of the materials, the minerals being "grouped according to their most prominent basic constituents, and preceded by a brief description of the chemical and physical characters of the leading base in each group."

In the illustrations there is a novel feature:—The manner in which crystals are drawn and their faces shaded is peculiar, the shading or stippling being made to show at a glance the mutual relation of the faces developed on compound forms. The classification of the silicates has been made to depend on the crystallographic systems to which they belong.

The work begins with a definition of minerals as distinguished from organic bodies, and an explanation of the difference between mineral species and rocks, showing the respective spheres

of the geologist, the mineralogist, and the chemist. We find next a condensed account of chemical composition, of the elements, of acids, bases, and salts, and of isomorphism. The second chapter teaches—as far as it can be attempted in a compass so brief—the use of the blowpipe. The physical properties of minerals form the subject of the next two chapters. To this succeeds an account of crystallography, followed by a systematic description of mineral species, forming the main portion of the work. The sources and occurrence of the various minerals are, for the most part, carefully given, though such localities as "Siberia," "Australia," and "United States," must be considered rather vague. Precision in this matter is of great importance in every department of Natural History. Even in case of widely distributed forms it is safest to state the exact spot where they have been met with. In speaking of gold, the author gives Tasmania and Van Dieman's Land as two distinct regions. English and American localities are generally placed as far as possible apart from each other, to prevent the confusion which the similarity of names is apt to occasion. The percentage amount of sulphur in iron pyrites is given as 63,—probably a typographical error.

Principles of Mechanics. By T. M. Goodeve, M.A., Barrister-at-Law, Lecturer on Applied Mechanics at the Royal School of Mines. London: Longmans, Green, and Co. 1874.

As Prof. Goodeve's "Elements of Mechanism" has so long been established as a standard text-book, we were prepared to receive favourably another work by the same author on a kindred subject. Nor are we disappointed. A glance at the present volume, which forms one of the series of Text-Books now being issued by Messrs. Longmans, is sufficient to show that it will help to sustain the high character which the mechanical and physical part of this series has already acquired.

As Lecturer at the Royal School of Mines, Prof. Goodeve knows well how to treat his subject. The student is made, at the outset, to realise the idea of Force, and to understand the modes by which it may be measured. At an early stage he is brought to grapple with some of the modern views which have been introduced into mechanics, such as the mechanical relations of the theory of heat. The laws of motion and the general principles of dynamical and statical science are clearly enunciated and copiously illustrated. Indeed, the characteristic feature of the present work is the constant reference which is made from the principles of the science to its practice. No sooner has the author laid down a principle than he proceeds to apply it by describing some useful examples, which exhibit its practical application. Hence we find in this work notices of many

inventions which we hardly look for in an ordinary treatise on the Principles of Mechanics. We may substantiate our statement by reference to the descriptions of Giffard's Injector, Blake's Stone-Crusher, the Moncrieff Gun-Carriage, Whitworth's Measuring Machine, and many other ingenious mechanical inventions. In fact, the author constantly seeks to interest the student by keeping the practical phase of the subject before him. Prof. Goodeve evidently has no notion of a student learning Mechanics by merely mastering abstract principles and acquiring dexterity in the application of formulæ. If such knowledge is to be any real service to a man, it must be supplemented by practical knowledge, such as may be gained in the workshop. "Mechanics," says Prof. Goodeve (p. 40), "cannot be learnt from books alone. The student must go out into the world, and see how mechanicians and engineers accomplish what they do; he must continually reason upon what he sees, and, retaining a firm hold of mechanical principles, he may thus gradually obtain a knowledge and mastery of his subject."

Elements of Metallurgy. A Practical Treatise on the Art of Extracting Metals from their Ores. By J. ARTHUR PHILLIPS, M. Inst. C.E., F.G.S., F.C.S., &c. Illustrated by numerous Engravings on Wood. London: Charles Griffin and Co. 1874.

RATHER more than twenty years ago Mr. Phillips wrote an excellent "Manual of Metallurgy," which originally formed one of the volumes of the "Encyclopædia Metropolitana." This manual passed through three editions, and a fourth has been long expected. Instead, however, of publishing a new edition of this Manual, the author has followed the far preferable course of presenting us with an entirely independent work. It is proverbially injudicious to put new wine into old bottles, and it has too often been found that to insert a quantity of new matter into an old text-book produces a result which is the very reverse of satisfactory. We are therefore not sorry to see the older work superseded by one which treats the entire subject from a modern point of view, and which may be thoroughly relied upon for giving its information quite up to date. Such a work was much needed, and the need could hardly have been better supplied than it has been by Mr. Phillips.

After a brief sketch of the history of Metallurgy, the author describes the various physical properties of the metals, and then enters into a detailed description of fuels and fire-clays. This is followed by a full notice of each metal, and the methods employed for its extraction, the several metals being treated in the following order:—Iron, cobalt, nickel, aluminium, copper, tin, antimony, arsenic, zinc, mercury, bismuth, silver, gold, and

platinum. The details of the metallurgical operations are described with great clearness, and the author's practical experience is sufficient guarantee that space is not wasted by the description of processes which are either obsolete or impracticable. It should be remarked that the illustrations are in most cases drawn to scale, and are consequently of great value to those practically engaged in metallurgy.

In the Preface to this work Mr. Phillips tells us that his object has been to supply such practical information on general principles and typical processes as may not only afford a comprehensive view of the science and art of Metallurgy, but will also enable the reader to study with advantage more elaborate treatises and original memoirs on special departments of the subject. This being the end which Mr. Phillips has set before him, he deserves to be heartily congratulated on its successful realisation.

Le Monde Microscopique des Eaux. Par JULES GIRARD. Paris: J. Rothschild.

A POPULAR treatise on the marvels, animal and vegetal, of pond-life, as revealed by the microscope. The remarks on the construction and use of this instrument err, if at all, on the side of brevity. The work is furnished with a great number of illustrations, some of which, however, are deficient in that precision and accuracy of detail which can alone give any real value, whilst others—*i.e.*, a general view of the vegetation on the margin of a ditch—are rather ornamental than useful.

PROGRESS IN SCIENCE.

MINING.

It might naturally be expected that at the recent meeting of the British Association considerable interest would be created by the iron-mines of Antrim. Within the last few years workings have been opened in all directions throughout the county; and indeed this remarkable development of iron-mining forms an important chapter in the industrial history of the North of Ireland. Not to mention the early notices of these ores, even in the beginning of the seventeenth century, we may remark that the ferruginous bands in the basalt of the Giant's Causeway were observed by the Rev. Dr. Hamilton, and specially mentioned by him, in 1790. No workings, however, were established until 1861, when Dr. Ritchie, of Belfast, opened up the deposit of ore at Ballypalidy, near Templepatrick. Brought into the market under the name of "Belfast aluminous ore," this mineral became largely used for mixing with other ores, especially with the rich hæmatites of Cumberland and Lancashire; the high percentage of free alumina in the Belfast ore acting as a capital flux to the siliceous hæmatite, and contributing to the formation of a good free-flowing slag. The composition of this Irish ore brings it into close relation with the mineral known as Bauxite, which occurs in the South of France and in Carniola, and is employed both as an ore of aluminium and as a fettling material for puddling-furnaces. The remarkable deposits of iron-ore now worked in other parts of Antrim differ for the most part from those of Ballypalidy, and often present a curious pisolitic structure; the spheroids, which are sometimes magnetic, being embedded in a matrix of brown or reddish iron-ochre. This pisolitic ore contains from 30 to 65 per cent of metallic iron, whilst the aluminous ore yields from 20 to 28 per cent of iron: even the ochres and lithomarge, though containing much less iron, have also been used in the blast-furnace. The beds of pisolitic iron-ore appear to lie all on one geological horizon, and thus divide the great series of basaltic rocks into two well-defined groups—one below and the other above the level of the iron-ores. It is probable that the Miocene ores of Antrim may have been formed originally from the products of decomposition of the basalt, and the deposition of these products under lacustrine conditions. The characters and distribution of these ores have been well described by Mr. Ralph Tate and Dr. Sinclair Holden, in the "Journal of the Geological Society;" by Prof. Hull, in "Iron;" and by Mr. R. A. Watson, in the "Dublin University Magazine." A brilliant future for the north-eastern corner of Ireland may be foreseen in the development of these important iron-making resources.

Among the papers read at the recent meeting of the Iron and Steel Institute, at Barrow-in-Furness, we may refer specially to one of local interest, by Mr. P. Würzburger, of Dalton-in-Furness. This memoir, "On the Geology of the West Coast Iron Districts," described the structure of the country which the Institute was then visiting, and dwelt especially on the mode of occurrence of the red hæmatite, which forms the staple of the great iron-manufacture of this district. These ores are found partly as veins in the Lower Silurian rocks, but mainly as deposits in the carboniferous limestone. The ore in the limestone occurs under several conditions: sometimes in flat deposits, following more or less closely the dip of the strata; sometimes in veins, with an inclination greater than that of the enclosing rock; and in other cases in irregular deposits, filling hollows or caverns in the limestone. In the Furness district most of the deposits are covered with superficial drift, but around Whitehaven they are generally enclosed in the solid limestone. In consequence of the great irregularity in the distribution and size of the ore-deposits the mining operations are rather precarious; borings in search of the ore are made at random, and it is impossible to estimate, with anything

like an approximation to accuracy, what extent of hæmatite still remains unwrought.

It may be well to remark that a description of the hæmatite deposits of Whitehaven and Furness, by Mr. J. D. Kendall, was laid before the Geological Society of London at its last meeting.

Whilst the occurrence of valuable hæmatite in Furness has led to the establishment of a vast industry in this district, it is to be regretted that coal has not been found in the neighbourhood of the ores. Search has been made from time to time, but hitherto without success. The most promising of these undertakings is that at Rampside, where a boring was commenced about five years ago, and has been energetically prosecuted in the face of great difficulties. Mr. Alexander Brogden, M.P., read a paper on this boring to the Iron and Steel Institute. For some time past the diamond-boring machine has been at work here, and at present the bore-hole has reached a depth of 1450 feet.

Irish coal-mining is carried on to so limited an extent that we may fairly call attention to the Tyrone coal-field, which ought to be developed with great benefit to the North of Ireland. Mr. E. T. Hardman, of the Geological Survey, has recently described this field. It measures only about 2½ miles in length by 1¼ mile in width; but although of so small an area it contains about twenty-four seams, of which at least thirteen are workable. They all consist of highly bituminous coal, of true Carboniferous age; fire-damp is almost unknown in the pits. Mr. Hardman estimates that the field still contains from 30 to 40 millions of tons of coal. Iron-stones are associated with the coal, though not in sufficient quantity to admit of being profitably worked, but the fire-clays in the upper part of the measures are largely used for the manufacture of bricks and tiles.

At a meeting of the Institute of Mining and Mechanical Engineers, recently held at Cardiff, a paper of much interest, "On the Coal-Fields of South Wales," was read by Mr. Forster Brown, the President of the South Wales Institute of Engineers. After tracing the history of iron-smelting in this district, he described in detail the geological structure of this field. Practically the field is divided into two separate parts, by an anticlinal ridge running in a sinuous east and west course. In passing from one part of the field to another, a change may be observed from bituminous coal to anthracite: this change operates both horizontally and vertically; thus, in passing from east to west the coal becomes, as a rule, more anthracite, whilst in other cases the upper seams in a vertical section may be more bituminous than the lower. Mr. Brown contrasted the modes of working coal in South Wales with those followed in the North of England.

To the same meeting a paper "On the Coal-Fields and Mining Industries of Russia was communicated by Mr. J. B. Simpson, who had recently visited some of the Russian collieries. The coal-fields of Russia may be referred to three districts:—the Tula coal-field, at present but little worked; the Donetz field, embracing a great thickness of good coals, partly bituminous and partly anthracitic; and a long narrow coal-field at the base of the Ural Mountains. None of the coal in these districts is of true Carboniferous age, and indeed the only coal-measure fuel belonging to Russia is to be found in a small, but productive, basin in Poland—perhaps an extension of the coal-field of Upper Silesia.

Attention has been directed, by Mr. P. Le Neve Foster, jun., to the coal-fields of Italy. It appears that Italy does not possess any coal of the true Carboniferous period, but many of the tertiary lignites are of very superior quality, and consequently well worth the expense of working. Mr. Foster has examined and described the coal-fields of the Tuscan Maremma, which are split up into a series of small basins by intrusions of eruptive rocks.

Three Reports on the Coal-Fields of Victoria have been drawn up by the Board which was appointed some time ago, by the Minister of Mines, to investigate and report upon the coal resources of the Colony. The fields of

Loutit Bay, Apollo Bay, and the Wannon, have been examined, but the result is by no means encouraging. Mr. Mackenzie, the Government Examiner of Coal-Fields in New South Wales, has reported that—in spite of what may have been said to the contrary—no workable seam of coal has yet been opened up in any part of Victoria. The Board consequently does not feel justified in recommending the expenditure of more money in boring for coal. The most important fuel in the Colony appears to be the lignite of Lal-lal, where a deposit occurs not less than 100 feet in thickness. This lignite has been described before the Geological Society of London, by Mr. R. Etheridge, jun.

But if Victoria is not blessed with coal, it has extraordinary treasures in its gold-fields. A Report on the Mineral Resources of Ballarat, by Mr. R. A. F. Murray, has been issued by the Geological Survey of Victoria. Not only is the geological structure of the country described, but the intricate system of auriferous "leads" has been worked out with much care. The gold-bearing drifts, associated with volcanic rocks which have been erupted at different periods, are classified under four heads. In connection with these drifts it is interesting to study the distribution of the quartz-reefs. It may be regarded as established that the supply of alluvial gold was derived from veins disintegrated during the various drift-periods, and that the gold—except in very fine particles—has not travelled far from the spot where it was separated from its parent-rock. In the district around Ballarat the richest ground is always in the neighbourhood of the quartz-reefs.

METALLURGY.

At the recent meeting of the Royal Cornwall Polytechnic Society a good deal of interest was excited by Mr. King's Patent Magnetic Ore Separator. This apparatus is used for the separation of minerals containing iron, in cases where the iron-ore is associated with other minerals. It is at present employed at the Ballycorkish Mines, in the Isle of Man, where the ore consists of a mixture of galena, blende, and spathose iron-ore. The lead-ore may be readily separated by taking advantage of its high specific gravity; but the blende and spathic ore, being of nearly the same density, could not be properly separated by even the best systems of sizing and mechanical dressing. Hence the introduction of the Magnetic Separator. The stuff, after being crushed, is roasted at a dull red heat in revolving retorts, when the carbonate of iron is decomposed, and a magnetic oxide produced. The roasted ore is then transferred to the hopper of the magnetic apparatus. This consists of a large drum or wheel, about 18 inches in diameter and 10 inches in breadth, furnished within with a system of magnets arranged radially. The mixed ore, in its passage over a series of four of these drums, has its magnetic portion gradually separated by attraction, and the part which escapes the magnetic drum is clean blende. Mr. King proposes to apply his system to the separation of tin-ore from the iron-pyrites, copper-pyrites, and mispickel, with which it is frequently associated in our Cornish mines. When these mixed ores are roasted, the pyrites may be transformed into magnetic pyrites.

"Our Tin Production and Tin Trade" formed the subject of a valuable paper, by Mr. Robert Hunt, F.R.S., recently contributed to the Miners' Association. According to the returns of the Board of Trade, the exports of tin of British production was 114,201 cwts. in 1871, 113,871 cwts. in 1872, and 115,946 cwts. in 1873. Of tin of foreign and colonial production we exported 41,196 cwts. in 1871, 48,634 cwts. in 1872, and 28,869 cwts. in 1873. Cornwall has now to compete not only with the tin production of "the Straits," including the Malacca Peninsula and the islands of Banca and Billiton, but also with the newly-discovered tin districts of Queensland, New South Wales, and Tasmania.

Mr. Crampton's revolving furnace for puddling iron was described by the inventor in a paper recently read before the Iron and Steel Institute. Small coal or slack is utilised as fuel, and is introduced with a current of air in such

a way as to ensure perfect combustion, and the consequent absence of smoke. The fuel and air are fed automatically into the revolving furnace in which the puddling is effected, and therefore without the mediation of a separate combustion-chamber. A very intense, though regular, temperature is obtained, and the phosphorus and sulphur are said to be eliminated to a very large extent from the pig-iron. The furnace is recommended not only by its economy of fuel, but also by the rapidity with which the puddling is effected. With a furnace 12 feet in length and 6 feet in diameter, the inventor has puddled pig-iron in an hour and a quarter from the time when it was cold. The revolving portion of the furnace is protected by a water-casing, and the difficulties of contraction and expansion incident to other revolving furnaces are said to be completely conquered.

The other papers bearing on Metallurgy brought before the Institute were for the most part on mechanical subjects. Among these we may mention one by Mr. Holley, of New York, descriptive of the general arrangement and principal details of the plant used in America for rolling steel rails.

MINERALOGY.

In compliment to the late M. Rivot, of the Ecole des Mines, the name of *Rivotite* has been bestowed upon a new mineral, which occurs in irregular masses disseminated through limestone on the west side of the Sierra del Cadi, in Lerida, Spain. It is an amorphous, compact, opaque mineral, varying in colour from yellowish-green to deep greyish-green. From its analysis the following formula may be deduced:—$Sb_2O_5+4(Cu,Ag)O$.

The memory of Dr. Livingstone is to be perpetuated in a new mineral species found at Huitzuco, in the State of Guerrero, Mexico. *Livingstonite* resembles ordinary antimony-glance, or stibnite; but with its lead-grey colour gives a red powder, and is thus distinguished from the antimony-ore. Moreover, M. Mariano Barcena, who has named the species, found it to contain 10 per cent of mercury, and it appears, indeed, that Livingstonite is a double sulphide of mercury and antimony.

Guanovulite is the name under which Dr. F. Wibel proposes to distinguish a pale yellow crystalline body which occurs associated with the eggs sometimes found in deposits of guano. Analysis shows it to be a hydrous sulphate of ammonia and potash, with an acid sulphate of potash.

Prof. Vom Rath, of Bonn, has described a new zeolitic mineral from the granite of the Isle of Elba. *Foresite*, as the new mineral is called, was found near the village of San Piero in Campo, and occurs in association with other zeolites, such as stilbite and heulandite. It is a hydrous silicate of alumina, lime, and soda, crystallising in the orthorhombic system. The name is complimentary to Sig. R. Foresi, of Portoferrajo, who has done much to aid in keeping up an interest in the mineralogy of Elba.

Under the name of *Veszelyite* a new mineral has been described by Prof. Schrauf, of Vienna. It is a hydrated phosphate of copper, presenting a bluish-green colour, and crystallising in the triclinic system. The mineral occurs on garnet, at Morawitza, in the Banat.

In continuation of Mr. E. S. Dana's researches on the crystallography of *Datholite*, he has applied himself to the study of the fine specimens in the K.K. Hof-Mineralien Kabinet at Vienna, and has published his studies in Tschermak's "Mittheilungen."

A valuable series of mineralogical contributions has been submitted to the German Geological Society by Herr Max Bauer, of Berlin, and published in the Society's "Zeitschrift." These studies relate to the rarer forms of garnet, to the physical properties of mica, the optical characters of margarite, the twin-striæ of iron-glance, and some peculiar crystals of smoky quartz from Switzerland.

ENGINEERING—CIVIL AND MECHANICAL.

The past season has, as is usual at that time of the year, been devoted to the meetings of Associations, at which engineering science has been largely represented. Section G of the British Association is devoted to mechanical science: a considerable amount of what is interesting to mechanical engineers finds its place in papers read at the autumnal meeting of the Iron and Steel Institute, as well as at the meetings of the Mechanical Engineers.

British Association.—Professor James Thompson, President of Section G, drew attention in his Address to the important question of railway accidents and the means of their prevention, and he traced the gradual development of progress in mechanical improvements as applied principally to points and signals. As this will form the subject of a separate article in a future number of the "Quarterly Journal of Science," we shall not dwell further upon it on the present occasion. From thence the Professor referred to the subject of steam navigation, the rapid progress of which he attributed mainly to the introduction jointly of the screw propeller, the compound engine, steam jacketting of the cylinders, superheated steam, and the surface condenser. The invention of the screw propeller has enabled steam to be used as an auxiliary motive-power in ocean steamers, for which paddles were inapplicable, whilst the other improvements have tended to an economy in fuel—an essential object in long voyages,—and by the use of which the consumption of coal is often now found to be reduced to about 2 lbs. per indicated horse-power per hour, from having been 4 lbs. or 5 lbs. in good engines in times previous to about twenty years ago. Deep sea sounding was next reviewed, and Sir William Thomson's new machine referred to, with which soundings have been made in the Bay of Biscay, and a specimen of the bottom brought up from a depth of 2700 fathoms, or a little more than three miles. One important feature in this machine consists in a friction brake arrangement, by means of which the arrival of the sinker at the bottom is indicated very exactly on board the ship. The illumination of lighthouses was next considered, the improvements in which were briefly explained. The practicability of employing gas has been one great movement in advance, and Sir William Thomson has recently succeeded in perfecting a self-signalling apparatus, by which different lighthouses may readily be recognised and distinguished from one another, and which is about to be adopted by the Belfast Harbour Commissioners. The Dublin harbour works carried out by Mr. Bindon Stoney, with concrete masonry blocks of 350 tons weight each, to which Mr. Thomson referred, has already been explained in Engineering Chronicles on a former occasion. From these subjects the Professor next branched off into the regions of sanitary engineering, wherein he dwelt principally upon the defective drainage and ventilation of our buildings, and the smoke nuisance of our towns. With reference to the latter subject he pointed out the economy that resulted from the use of well-known mechanical contrivances for the more effectual combustion of coal, and he instanced a case where one method is applied to about thirty ordinary 40 horse-power boilers, in which upwards of 100 tons of coal are burned daily, and from the chimneys of which not more smoke is emitted than from many a kitchen fire. By the adoption of this method the bed of coal, which is gradually supplied in front, is caused to travel along the bars towards the inner end of the furnace, and the combustion proceeds in a very uniform manner in conditions highly favourable to economy of fuel, and without the emission of almost any visible smoke.

We shall notice two papers that were read before this Section, viz., "On the Upper Bann," by John Smyth, and "On the Eclipsing Apparatus at Holywood Lighthouse," by Mr. W. Bottomley.

The former paper gave an account of recent works undertaken for improving the water supply of the river Bann for the mills which are established on its banks; for this purpose, the Bann Reservoir Company was formed in 1835, and by its means the Loughislandreavy reservoir was finished in 1839, and the Corbet reservoir in 1847. Loughislandreavy reservoir is situated in a narrow valley in the flank of the Mourne Mountains, and receives three-fourths

of its supply through a conduit or feeder from the Muddock River and one-fourth in the same manner from the Moneyscalp river. The water is impounded to a depth of 35 feet above the old lake which existed there, and can be drawn off to a depth of 38½ feet below top water. The area of the old lake was 93 acres, and that of the present reservoir 250 acres, and the capacity 270 million cubic feet. The embankments are substantially constructed, and protected against the wash of the water by strong stone pitching. An important and most successful speciality in their construction was the use of peat as a supplementary aid to the puddle. The water is discharged through two 18-inch iron pipes, secured in a culvert built in the solid ground beneath the main embankment, and is conducted by an open channel to a point about 1 mile down stream from where the supply is lifted. The water from the reservoir reaches the Bann after flowing down the Muddock for 6 miles. From the mouth of the Muddock to the weir, where the surplus water of the Bann is lifted for the Corbet reservoir, there is about 40 feet of unoccupied, and 7½ feet of occupied, fall on the Bann. At this weir the care-taker daily measures the quantity of water coming down the river, and thereby regulates the amount to be supplemented by the reservoirs. The channel of the feeder from this weir to the Corbet reservoir is about a mile long, and wide enough to take in considerable floods. The area of the reservoir is about 70 acres when the water is at top level, or 11½ feet above discharge outlet. The water is discharged through small iron sluices into a conduit communicating with the river. The cost of these works has been about £30,000.

In the paper upon lighthouse illuminations the author observed that in order, under present arrangements, to make out with certainty what any observed light is, it is necessary that the master of the vessel shall first ascertain the position of his own ship. In many cases this cannot be done even in short voyages, but after a long voyage, and with but few opportunities for making correct observations, errors of many miles may occur in a ship's reckonings. Every year the accounts of shipwrecks show the fatal results arising from the mistake of one light for another light many miles away: the signal which properly interpreted should have preserved the mariner from danger, misinterpreted becomes the false guide which lures him to destruction. Even coloured lights do not afford the required protection, but what is required is that each light should unmistakably declare its own identity; and the plan for effecting this, which was first proposed by Charles Babbage, in 1851, and has recently been perfected by Sir William Thomson, is that each lighthouse shall exhibit from sunset to sunrise a certain definite series of eclipses representing one of the letters of what is known in telegraphy as the Morse alphabet. This method has recently been adopted by the Harbour Commissioners of Belfast for the light on the Holywood Bank, where, by the use of three shutters, or screens, two short eclipses and one long eclipse are given, corresponding to the letter U in the Morse alphabet, and the system will therefore receive a fair trial.

The majority of the papers read before the Iron and Steel Institute at their meeting at Barrow-in-Furness belonged more to geology than engineering, but two of them may be classed under the latter heading—one being a paper by Mr. Robert Luthy, "On Valves Suitable for Working Hydraulic Machinery;" and the other, by Mr. T. Wrightson, of Stockton-on-Tees, "On a New Form of Wagon Drop for Blast-Furnaces."

In the former paper it was explained that under high pressures the old-fashioned taper, or circular, valves are not reliable; brass slide valves of the usual D pattern, similar to steam-engine slide valves, do very well for smaller sizes, and if their working faces are made of hard metal they last a considerable time; but for the larger sizes the friction becomes so great that, in order to work them by hand, a very great leverage has to be employed, and this prevents the valves being opened or closed quickly enough. Mitred plug valves are used in some places, but they are complicated and expensive, and require a great deal of room. The foregoing arrangements of valve have all metal faces, but recently valves have been introduced in which the peculiar properties of the leather collars for making hydraulic joints have been applied

to the best advantage. The leathers, it is found, last a very long time, and if a valve has to be examined, or fresh leathers put in, it can be done in a few minutes, whereas the repair of metal-faced valves is both tedious and expensive.

In the ordinary form of wagon drop a frame-work, usually of cast-iron columns, braced well together, supports an entablature, on the top of which is mounted a strong shaft with two large sheaves keyed thereon, to one or both of which is applied a powerful brake, worked by a lever from the upper rail level. The cage moves up and down in guides fixed to the frame-work, and is suspended by chains or wire ropes descending from one side of the sheaves, whilst from the opposite side hang heavy counterweights, which are sufficiently in excess of the weight of the cage to draw it to the top when the wagon is not on. Instead of weights Mr. Wrightson proposes to use water as the controlling agent in the drop. It would occupy too much space to enter into a description of how this is arranged, but it will be readily understood by anyone acquainted with the general principles of hydraulic machinery.

Mr. F. J. Bramwell, as President of the Institution of Mechanical Engineers, recently delivered a very remarkable address on the performances of his profession and the progress made in mechanical science, from which it would seem that the mechanical engineer is necessary to our very existence, and that few modern improvements could have been adopted without his aid, and yet he urged that there was no reason for supposing that the time had arrived when there need be any slackening in the development of engineering science, for there remain vast fields of research in which the ground has as yet scarcely been broken, while there are others practically untouched, and, even in those departments of engineering in which most progress has been made, far more remains to be done than has yet been accomplished. The waste of fuel was dwelt upon, and Mr. Bramwell remarked that for years past there have been constructed steam-engines capable of developing all the power at present obtained by the use of such engines, with the consumption of but little, if any, more than one-third of the fuel actually expended for that purpose, while in our metallurgical and manufacturing processes parallel instances exist. Attention was also directed to the transmission of power over long distances, for which purposes the chief methods adopted are by a fast running rope, by the exhaustion or compression of air, or by the flow of water through pipes under pressure. Amongst other subjects dwelt on were the substitution of mechanical for human labour; the mode of joining materials, and the abvisability of devising some mode less crude than the present practice of employing bolts and rivets; on the utilisation of so-called "waste" products; on the advantages which the present generation of engineers may be expected to derive from the spread of technical education, and on the disadvantages which, on the other hand, are attendant on the modern tendency towards subdivision in engineering manufactures.

The first paper read was one by Mr. John McConnochie, of Cardiff, "On the Bute Docks and the Mechanical Arrangements for Shipping Coal." The dock accommodation at present existing affords a water area of 77 acres, to which will be added a further area of 54 acres when the new Roath Dock is completed. As the tidal water at Cardiff carries a deal of silt in suspension, and is therefore not suited for supplying the docks, they are fed with water drawn from the river Taff about 2 miles above them. In connection with the harbour accommodation there are at present four graving docks. The lock gates, as well as a large swing bridge over a junction lock, and the various hoists, tips, &c., connected with the new basin, are all actuated by hydraulic machinery supplied by Sir W. G. Armstrong and Co. As the preponderance of the trade of the port consists of exports, vessels arrive in ballast, and there are seven steam cranes specially employed in the discharge of this ballast, four of which are capable of discharging 200 tons each per hour. These steam cranes discharge into railway wagons, by which the ballast is conveyed a distance of about 2 miles to spare land, where it is deposited. The mechanical arrangements for shipping coals at the West and East Docks are on the high level, or balanced principle, but at the New Basin the low level system has been adopted into hydraulic tips for elevating and lifting the

wagons. The provision for loading coals consists of thirty-one balance tips and twelve hydraulic tips, or forty-three in all; and, as each tip is capable of shipping 560 tons of coal per day of ten hours, the total shipping capacity of the Bute Docks is nearly equal to 8 millions of tons per annum.

We have to record the death, during the past quarter, of more than one eminent member of the Engineering Profession, amongst whom we may note Mr. T. Marr Johnson, Mr. John Grantham, Sir Charles Fox, Sir John Rennie, and Sir William Fairbairn. Sir William Fairbairn was born at Kelso, on the 9th of February, 1789, his parents occupying a comparatively humble position in life. His first occupation was at the age of fourteen, when he obtained employment on the new bridge at Kelso, which was being erected by Mr. Rennie. Afterwards he was employed by his father on the Percy Main Colliery, of which he was manager, and at the age of sixteen he was apprenticed to the Colliery Company, and he commenced a course of self-education. At the age of twenty-one years he found his way to London, where he first obtained work at Grundy's Rope-Factory, at Shadwell, and afterwards was engaged by Mr. Penn, at Greenwich. Afterwards he worked at the Phœnix Foundry, Dublin, and in 1814 he made his way to Manchester, where he settled as a working millwright under Mr. Adam Parkinson. After two years he married, and commenced business on his own account, one of his first efforts at engineering designing being the plans for an iron bridge over the River Irwell, at Blackfriars. Fairbairn gradually rose to a position of eminence, and in 1831 he constructed one of the earliest examples of iron ship-building, the success of which led to the establishment of the well-known works at Millwall. He was engaged with Robert Stephenson in designing and constructing the Britannia Tubular Bridge. In course of time the firm of which Sir William was the leading partner was turned into a Limited Liability Company, and one of the latest works of importance turned out by it was the construction of the iron forts for the defence of Spithead.

GEOLOGY.

Glacial Geology.—The glaciation of the South-West of England has lately attracted some attention. Mr. Croll, speaking of the Great Baltic glacier, 1000 or 2000 feet in thickness, is of opinion that if it be admitted that it passed over Denmark,—and of this there is good geological evidence,—then it is hardly possible to escape the conclusion that a portion of it at least passed across the South of England, entering the Atlantic in the direction of the Bristol Channel. Mr. W. C. Lucy has detected Glacial striæ on a mass of sandstone near Porlock. Mr. H. B. Woodward has recorded the occurrence of Boulder Clay near Yarcombe, on the Black Down hills. Looking to the nature of the deposits found on these hills, he thinks that their formation may be attributed to marine action during the glacial submergence, with the assistance of an occasional iceberg to bring the foreign material (quartz and quartzite) which is mixed with the flint and chert drift.

Mr. T. F. Jamieson has arranged the Glacial phenomena of Scotland under three heads:—(1), the great early glaciation by land-ice; (2), the period of glacial marine beds containing remains of Arctic Mollusca, when most of the country was covered by the sea; (3), the time of the late glaciers, the special subject of the paper. He described this last period as one not of mere local glaciers, but as characterised by a return of a great ice-sheet over nearly the whole of Scotland and Ireland; and he stated that this ice-sheet was probably neither so thick, so extensive, nor so enduring as that of the first period of glaciation, which cleared away everything in the shape of superficial deposits down to the hard rock. He believed, however, that in the last period the mountains of Scotland and Wales, as well as the Pennine range and the rest of the North of England as far as Derby, were covered with thick ice, which in most parts reached down to the sea, and that extensive snow-beds prevailed over the rest of England. In the summer months the melting of these would give rise to streams of muddy water, and produce the superficial deposits of brick earth, warp, and loess; whilst, when the currents were stronger, perhaps

from the thaw being unusually rapid, deposits of gravel would be formed. This second ice-sheet would gradually become less, and break up into valley-glaciers, which in their retreat would leave kaims and eskers at low levels, and moraines in the mountain-glens. During this time no new great submergence of the country took place; and the last great modifications of the surface were subaërial, and not submarine, the work having been done by frost, rain, and glaciers.

Mr. J. G. Goodchild has described the Glacial phenomena of the Eden Valley and the Western part of the Yorkshire Dale district. He considers that they could not possibly have been produced by floating ice, and therefore must have been caused by land-ice. The flow of ice evidently came from the Scottish southern uplands, and it could not well have exceeded 2400 or 2500 feet in thickness.

Stratigraphical Geology.—The exact relations of the Keuper and Bunter divisions of the Trias, in the British Isles, have never been fairly established. Mr. J. Anderson points out that in County Down they appear conformable, whereas near Nottingham the Keuper is said—by the Rev. A. Irving—to rest upon the eroded surface of the Bunter. Mr. Irving has noticed clear signs of continuous deposition of the Permian and Lower Bunter rocks in the Nottingham district; and this fact is important, considering that one series of rocks is classed as Palæozoic, the other as Mesozoic. Mr. H. B. Woodward, referring to the Red Rocks in the South-West of England, remarks upon the continuous series of rocks from the breccias and sandstones of Dawlish and Teignmouth up in to the Rhætic beds of Axmouth. Although the lower portion of these Red beds has been called Bunter, and the upper Keuper, there is no unconformity, and the Muschelkalk may, he thinks, be represented by sediments of a different lithological nature to the continental beds. Nevertheless it is not well to make any subdivisions in this part of England equivalent to those on the Continent, but rather to rest satisfied with terming the whole series the Trias.

Mr. H. G. Seeley having visited the Devonian country in search of the fault which the late Mr. Jukes supposed to traverse it, records his opinion that no such fault exists. He thought that Mr. Etheridge's detailed grouping of the rocks was better suited to the north-west part of the country than West Somerset, and that for that region the divisions of strata used by Mr. Jukes were convenient.

Palæontology.—Mr. E. B. Tawney has described and figured nineteen new species of *Gasteropoda* from the Inferior Oolite of Dundry Hill, near Bristol. He also adds fifteen more species to the list of British fossils: altogether he records sixty-six species in a determinable condition.

Dr. Nicholson has described a new genus of tabulate corals, *Columnopora*. intermediate between *Favosites* and *Columnaria*, of which only a single species has been obtained from the Hudson River Formation of Ontario and Ohio.

The occurrence of Labyrinthodont remains in the Yoredale rocks of Wensleydale has been noted by Mr. L. C. Miall.

Prof. Young and Mr. John Young have described two new forms of Polyzoa from the Carboniferous Limestone shales near Glasgow, under the names of *Actinostoma* and *Glauconome*. Referring also to the genus *Palæocoryne* (described by Dr. Duncan and Mr. H. M. Jenkins), they maintain that the structures are not independent organisms, but mere processes of the Polyzoa on which they occur, the cells at the base being only the cells of the Polyzoa.

Mr. H. G. Seeley has recently described the tibia of a large struthious bird (*Megalornis*) from the London Clay of Eastchurch, in Sheppey. Mr. Seeley has also founded the genus *Ophthalmosaurus* for some reptilian remains from the Oxford Clay.

Sub-Wealden Exploration.—The boring has now reached a depth of upwards of 1000 feet. The beds traversed have been proved to belong to the Lower Wealden, Purbeck and Portland Series, and Kimmeridge Clay. The Oxford Clay has been last detected. The Committee—represented by Prof. Ramsay,

Mr. John Evans (President of the Geological Society), and Prof. Prestwich—make an earnest appeal for further assistance to carry on the work.

The Steppes of Siberia.—Mr. Thomas Belt has described some sections examined by him on a portion of the Siberian steppes, which have enabled him to form an opinion as to the origin of the great plain. The best section seen by him was at Pavlodar, which showed 50 feet of sand and silt, with occasional lines of pebbles. South of Pavlodar the surface of the ground was covered with pebbles, which became larger in advancing southward, until the soil was full of large angular quartz boulders. Further south the bed-rock comes to the surface in ridges and low hills, increasing in height until some of them attain 2000 feet. All the rock-surfaces were much shattered, as if by the action of frost, but they showed no signs of glacier-action. The generally accepted marine origin of the great plain was said to be negatived by the absence of sea-shells in its deposits, whilst *Cyrena fluminalis* occurs in them. The author regards them as deposits from a great expanse of fresh water kept back by a barrier of polar ice descending far towards the south. In its greatest extension this ice-barrier would produce the crushing of the bed-rock; and as it retreated, the water coming down from the higher ground in the south would cover a continually increasing surface. The distribution of the boulders on the plain north of the ridges was attributed to floating ice.

Professor of Geology at Oxford.—Mr. Joseph Prestwich, F.R.S., &c., has been appointed to the office of Professor of Geology in the University of Oxford, as successor to the late Prof. Phillips. The Council of the Institution of Civil Engineers have recently awarded to him a Telford Medal and Premium, for his paper "On the Geological Conditions affecting the Construction of a Tunnel between England and France."

Dr. F. Stoliczka.—Palæontological Science has lost one of its ablest students in the death of Dr. Stoliczka, at the early age of 34. He is known chiefly through his connection with the Geological Survey of India, by his descriptions of many of the fossil organic remains collected by the Staff. For several years he was Natural-History Secretary to the Asiatic Society of Bengal, and it is mainly to his exertions that this Society owes the resumption of much of its early vigour.

PHYSICS.

Light.—M. Rayet has published a "Note on the Spectrum of Coggia's Comet." On May 19 the light gave a continuous spectrum from the orange to the blue (spectrum of solid nucleus) traversed by three bright bands (spectrum of gaseous nebulosity); but the continuous spectrum was very narrow compared with the ordinary nuclear spectra; and the luminous bands, instead of being stumped towards the more refrangible side, terminated, towards red and violet, in very distinct straight lines. Father Secchi also writes: On June 18 and 19 the spectrum with the bands of carbon was considerably developed, the green band remaining the more distinct, whilst in the comet of Temple the yellow was brightest. This proves that the gaseous compounds are not the same in all comets. At the beginning of the month there was merely a spectrum of bands, now there is a general connecting line which unites them into one continuous spectrum. The bands of the comet are more diffuse than those of carbonic oxide. They resemble the spectrum obtained by the electric spark in the vapour of benzine.

A new helioscope has been constructed by M. Prazmowski. The idea of employing the polarisation of light in place of coloured glasses to diminish the lustre of the sun is not novel; but the quantity of light reflected, even under the Brewsterian angle for glass of a low index ($n = 1\cdot5$) is so considerable that the eye cannot support the light. The author obviates this difficulty by taking a rectangular prism of an index, n, and cementing upon its hypothenuse another similar prism, so as to form a cube. The index of the second prism is n'. A ray of light meets in its course the first hypothenuse of the cemented pair with an incidence of 45°; this is the surface whose index is $\frac{n}{n'}$. As $\frac{n}{n'}$ is

very nearly unity, the angle of 45° is very near the Brewsterian incidence. The instrument has been further modified by Janssen.

Microscopy.—Herr Möller has introduced a very ingenious modification of his celebrated *Diatomaceen typenplatte;* the old form had about four hundred species neatly arranged in the space of about a quarter of an inch square, each slide being accompanied by a manuscript catalogue. The new arrangement consists of a photograph about 4 millimetres square, of eighty circles, ten in a longitudinal and eight in a vertical direction; beneath each circle is the name of the object and its author, and in the centre of each of these circles is a diatom, and in many cases two are mounted in order to show front and side views. The whole collection independently of its great value to the student of Diatomaceæ is a marvel of manipulative skill.

Those who have attempted to dry the petals of *Tradescantia Virginica*, have probably noticed that the coloured cell contents break from their envelopes and flow towards the margin of the petals, leaving the greater part in a colourless condition. The petals and their alcoholic tincture giving an interesting absorption spectrum of three bands, it has been a desideratum to preserve the colouring matter in some permanent form for purposes of reference and comparison; the alcoholic tinctures of most flowers rapidly fade, and it is impossible to dry the very fleshy petals of *Tradescantia* in the ordinary way; but the colour has been successfully preserved by mounting in balsam by the "Oil of Cloves" process. The petals separated carefully from the flower so as to avoid breakage or bruises are immersed in ordinary methylated spirit and placed under the receiver of an air pump to ensure rapid displacement of the contained fluid and air; they are next transferred to absolute alcohol, the air pump being also used to save time, then placed in oil of cloves until transparent, and can then be mounted in balsam, taking care that as little heat as possible is employed. Petals of *Cineraria* and *Lobelia speciosa* also make good objects for the micro-spectroscope. The colours are much more brilliant than when dried petals are mounted in balsam, even in those plants which can be successfully dried by the ordinary process.

An exhibition of high scientific value took place at the meeting of the South London Microscopical and Natural History Club. This society has increased so much during the three years of its career that it has been found necessary to seek for a larger meeting-room, in which the first meeting was held on the 21st July. Instead of an indiscriminate collection of objects being exhibited, as at most *conversaziones*, the President, Dr. Braithwaite, F.L.S., determined to limit the exhibition to three subjects, viz., the cuticle of plants and its appendages of hairs, scales, glands, &c.; cyclosis or circulation in plants; and the hard tissues of fruits. A very complete series of preparations was shown, and in addition short lectures on each subject were delivered by the President and two other demonstrators conversant with their respective departments. The educational success of the exhibition was so marked that other meetings illustrating equally important points of minute structure will follow, and probably replace the old fashioned and almost useless *soiree*, which has hitherto been the only means of bringing the microscope and the minute structure revealed by its aid before the general public.

Dr. J. G. Richardson exhibited at the Academy of Natural Sciences of Philadelphia a new "Syphon Slide." It consists of a slip of plate glass 3 inches × 1 inch or other convenient size, in the upper surface of which is ground a shallow groove, elliptical both in its transverse and longitudinal section, and deeper towards one end than the other. This excavation is arranged so as to receive a small living fish, tadpole or other aquatic animal, and retain it under sufficient constraint to prevent any troublesome movements, and also to prevent any injurious pressure. The improvement over other slides of this description consists of imbedding a small metallic tube at each end of the cell, and adapting to each of these two tubes pieces of elastic pipe, one being intended for the entrance and the other for the exit of any fluid which it might be desirable to employ. For the examination of

larger animals similar cells of suitable dimensions can be constructed of larger and thicker pieces of glass. With such an apparatus a current of iced water can be passed through the cell, and any injurious effects which might be caused by the use of the electric- or lime-light entirely counteracted. Animals may be kept without injury in these cells for several days so long as a constant current is maintained. This contrivance offers many facilities for prolonged physiological investigations.

TECHNOLOGY.

Composition and analysis of the concentrated milk of the Anglo-Swiss Company of Cham has been determined by M. A. Müntz. The reaction of the milk is feebly alkaline, and its specific gravity is 1·313. Contrary to what has been supposed, a certain quantity of inverted sugar is present. The composition of two samples was—

	No. 1.	No. 2.
Cane-sugar	38·8	29·4
Inverted sugar	1·7	12·4
Milk-sugar	13·3	13·9
Butter	9·5	8·5
Casein, albumen, and salts	11·0	12·0
Water	25·7	23·8
	100·0	100·0

The amount of inverted sugar increases with keeping. The milks employed in the manufacture of these two samples must have had the following composition:—

	No. 1.		No. 2.	
Milk-sugar	5·2	13·2 solids.	5·2	12·8 solids.
Butter	3·7		3·2	
Casein, &c.	4·3		4·4	
Water	86·8		87·2	
	100·0		100·0	

These samples agree closely in their composition with a normal milk, which may be taken as—

Milk-sugar	5·2	13·0
Butter	4·0	
Casein, &c.	3·8	
Water	87·0	
	100·0	

The composition of this milk contrasts very favourably with that generally supplied in Paris, which frequently contains not more than 7 per cent of solids, of which 1·5 to 2 is butter.

According to Ch. Mène, Japan wax has been regularly imported into Europe for some years, and is quoted at from 1¾ to 2 frs. per kilo. It is extensively used for the adulteration of bees'-wax, the value of which ranges from 3¾ to 4 frs. per kilo. This fraud may be detected by the specific gravity of the sample. That of bees'-wax is 0·96931, that of Japan wax 1·00200. Yet all the mixtures containing from 50 to 90 per cent of Japan wax are lighter than bees'-wax.

M. d'Havrincourt collected cockchafers at the price of a franc per 10 litres, and having destroyed them with gas-liquor and sulphuric acid, obtained a good manure. A previous experiment, where the beetles were worked up with lime, gave unsatisfactory results.

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LIST OF PLATES AND WOODCUTS IN VOLUME IV. (N.S.)

ERRATA.

Page 378, line 1, *read* "motions of the upper currents and of the heavy masses."

Page 383, line 17 from bottom, *read* "It was at this point within 1½ miles of Iowa River, the course of which is here nearly parallel with that of the tornado. The crookedness," &c.

Page 394, line 13, *for* "Smithsonian Institute of Warsaw and Illinois," *read* "Smithsonian Institute, Warsaw, Illinois."

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